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THE OPTICAL AND PHYSICAL PROPERTIES OF MICA.

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GENTLEMEN: I have chosen the subject of "The Optical and Physical Properties of Mica" for my lecture this evening, because I have found the study of the optical and physical properties of that substance exceedingly interesting, and I have found it very hard to get any information about it from books. And therefore, as it is rather an obscure but an intensely interesting branch of optics, I thought it might be worth while to put together what I have learned, and to tell you about it.

In the first place, I suppose I need not do very much to emphasise the difference between mica and talc. This material (mica), which you all know, is transparent, thin, flexible, easily capable of being split into thin sheets, and is used nowadays for very many purposes, such as lamp shades, fronts of furnaces, and innumerable other services. It is also used in a variety of electrical apparatus as an insulating material. It is a material which is perfectly familiar to everybody, and yet people will persist in calling it *talc*. I have in my hand here a piece of real talc, and I think you will agree that a stone more optically different from true mica would be hard to imagine. This talc is soft, splitty, fibrous more or less, and if you examine the sample, you will see that it would be of precious little use to anybody for any optical purpose. Therefore I will say nothing more about talc, but confine my remarks entirely to the subject of mica.

Mica is the generic name given to a number of minerals, all of them essentially mixed silicates—silicate of magnesia, silicate of alumina or silicate of iron, along with potash, and other chemical elements, combined in certain relations rather than in certain proportions. They all agree in having this easily laminated character, in having a certain curious crystalline structure, and though they differ considerably in their compositions, they are all very nearly of the same specific gravity.

But they differ widely in optical properties, The names given by mineralogists to the different varieties I have put upon the board in the form of a table.

VARIETIES OF MICA.

	SP. GRAV.	AXES.	ANGLE.	INDEX OF REFRACTION.
MUSCOVITE (Pot., Sod.; Alumin.)	2·7 to 2·9	biaxial	55° to 75°	1·556 to 61
PHLOGOPITE (Pot., Hyd., Mag. Al, Iron)	2·72	biaxial	7° to 18°	1·562 to 1·606
BIOTITE (Mag., Iron; Alumin. Iron)	2·7 to 3·1	uniaxial?	0° or nearly	1·54 to 1·579
LEPIDOMELANE (Iron, Mang.; Iron, Al.)	2·9 to 3·1	uniaxial? and biaxial	?	?
ZINNWALDITE (Pot, Sod., Lith.: Iron, Mag.)	2·9	biaxial	50° to 67°	?
LEPIDOLITE (Lith., Pot., Sod.: Mag., Iron)	2·8	biaxial and uniaxial?	76° to 86° 0° or nearly	1·597 to 1·605

The most familiar kind, *Muscovite*, is that known in the middle ages as Muscovy Glass, and I have here a sample of the Siberian or Russian mica. This is the form in which it comes very often to this country from Russia, trimmed round with a sickle at the edge, and already split into fairly thin pieces, but capable of being split again and again. The natural form of it, before it has been split and dressed, is very different. Here I have a thicker slab of a natural crystal of Muscovite. You will see it has, generally, roughly speaking, a sort of six-sided appearance. It occurs in this way as rough crystals. These things are found in various parts of the earth, and are found of very different sizes. Here I have a little sample of granite containing mica, which I picked up last summer in Glen Sannox, in Scotland. Then I have another piece which is of a somewhat dirty appearance, with a contorted grain, and not at all transparent; here is another specimen which is fairly shiny, and here is a piece of micaceous schist which is really much more like the talc I showed you just now. Here I have a sample of mica which looks very much like snow, or powdered silver. I believe that has been heated in order to get it into those white silver flakes. And here is a material found native in Germany—I am not sure if this particular specimen has been artificially treated—but it is a golden mica which is flaked up in the same way and called by the Germans "Katzen-gold," or Cat-gold. Whether

you find it in small flakes mixed with other constituents like that Scotch specimen, whether in irregular lumps, in small crystals or large, it has the same peculiarity of being readily cleaved, in one particular direction, into thin sheets. It has been found in certain places of enormous size. In Virginia there was once found a crystal which weighed about a ton: it was nearly three feet across and more than a foot thick, but it could be cleaved into large sheets. Here I have a sample not of that particular piece, but a portion of one of those six-sided crystals, but clipped across at one side. It is nearly 20 ins. across. There is a slice, not very transparent, of a brown colour, and there are several others on the table here, intended simply to exhibit the crystalline form. The one I have now in my hand is a sample of Australian mica, not Muscovite, but a specimen of the last variety shown on my list, namely, *Lepidolite*, of a purple or peachy colour.

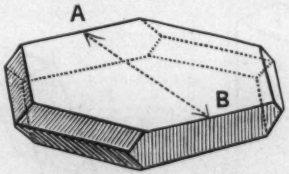

FIG. 1.—MODEL OF NATURAL CRYSTAL OF MICA.

Here is another sample of this same Australian Lepidolite, and here is a bit of brown *Phlogopite*. I have here samples of most of the different kinds of mica. Here for instance is a block of the kind known as *Biotite*, which is very flexible, but very splitty. It breaks up easily, and is not a very useful variety. Then there is a small specimen here of the very black kind known as *Lepidomelane*. It is black because it contains iron and manganese—principally iron, both in the ferrous and the ferric state. It is an exceedingly black sort, and is usually found in thin flakes. Of *Zinnwaldite* I have no sample; it is rather rare, but it is only interesting because it differs somewhat, externally, from the others. I have shown you the Lepidolite because it contains a large amount of the element lithia. All these, with the exception of Zinnwaldite, may be said to have a certain primitive form, the same form as this block of wood here (fig. 1) which, as you see, is a sort of six-sided form, but not exactly the same on all the six sides, two of them are straight, and the other four generally are more or less sloping.

We find, as a matter of fact, that the crystal has certain properties in the direction across from one straight side to the other, in which direction, marked AB in fig. 1, its properties differ from the properties in other directions. This you will learn presently when I say more about them.

Mica does not belong to the hexagonal system (fig. 4) of crystals, however. It was for a long time thought to belong to the rhombic system, that is to say, the system of which the primitive form is somewhat of the shape of a match box, with three different axes in three different directions at right angles to one another. But it is now known that it does not belong to the rhombic system (fig. 2)—it really belongs to the form called monoclinic, wherein two of the axes are at right angles to one another, and the third is oblique to the other two; as though the shape had been raked or tilted over (fig. 3). Now I suppose you

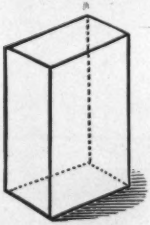

FIG. 2.—RIGHT RHOMBIC SYSTEM.

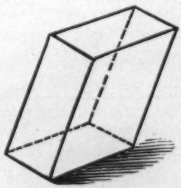

FIG. 3.—MONOCLINIC SYSTEM.

will say this hexagon does not look very much like a square block raked over on one side. No, perhaps not, but then remember the external form of the crystal is determined very often by its tendency to split. Take, for example, the common double-refracting Iceland spar. The crystalline form to which we should refer it as a system is the six-sided system, having a principal axis around which the other axes are grouped symmetrically. But, as a matter of fact, the natural planes of cleavage, which are three in number, are all three inclined to the axis of the hexagonal system. Consequently when you have split this up it splits up into rhombohedrons. It imitates, in one sense, the monoclinic form, but nevertheless it is, optically, a crystal having but one axis, and belonging truly to the hexagonal system. So, in the same way, this crystal mica, which belongs to the monoclinic system, imitates the hexagonal system by its cleavages. It has

various cleavages, but one particular plane, which is more pronounced than the other—the plane parallel to which it cleaves—is considered as its base, and we have, practically, to consider it only with respect to this plane of cleavage. Now the monoclinic and rhombic systems, both of them, as well as the hexagonal, are systems which, because they have these inequalities in their axes, are not symmetrical around and along their principal axes, as the other systems, the cubical, the tetragonal and the hexagonal systems, are. So not being symmetrical around one axis, the consequence is that the rhombic, the monoclinic, and the triclinic systems have two oblique directions through the crystal which are axes of maximum elasticity, with axes of minimum elasticity at right angles; and therefore they are optically biaxial. They will

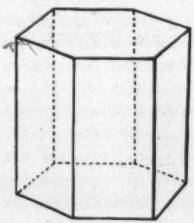

FIG. 4.—HEXAGONAL SYSTEM.

show in polarised light symmetrical patterns around the two axes instead of giving simple rings and crosses around one axis. Herein lies the easiest method of discriminating the different kinds of mica, as to whether they are biaxial or uniaxial, and if biaxial how wide apart the axes are from one another. I will come to that, perhaps, presently. Once more I will draw your attention to the general six-sidedness of the outline of this crystal. The rest of the samples in this group are intended to show you how this crystalline form re-appears in certain of the natural angles. Here is a piece for example —a piece with the natural six sides, and I think cut a little out of true in one case. Here is another piece which is, in fact, two put together naturally as a twin crystal. One of the difficulties in handling mica is that it is so apt to split, and, therefore, I must ask you much as you would desire to do so, to be very careful to try and not on any account split my crystals for me. Some of the very finest speci-

mens are not mine, they are kindly lent to me for this occasion by Messrs. Wiggins and Sons, the well-known mica merchants, and I should be very sorry if any accident happened to these specimens.

Passing away from the general structure to the question of colours, you will see that mica has many different tints. Some of these specimens are red, some brown, some green, some grey. Here is a sample of a very good quality of Muscovite, and here is a piece of comparatively rougher stuff. I will refer to that one again presently. These samples are to illustrate the differences of colour. Here is a very fine piece of greenish grey, nearly uniform in colour, and here is a piece nearly black, so dark that you cannot see through it. Here again is a piece which looks almost black on the surface—it is a kind of greenish bronze. Here is a very beautiful piece of grey, fairly transparent, and if it were thinner it would be quite transparent. Here is a piece of that Lepidolite from Australia, and here is a sample from India—I do not know whether from Bengal or Ceylon—of a kind called Port Wine Mica. It is of an exceedingly dark red colour like a ruby glass. The specimens of the next group are four in number; and they are to illustrate what so very often happens in the case of mica, namely, that included between the laminæ of the mica there are other substances which we call "inclusions." Here, for example, is a very beautiful piece illustrating inclusion: the dark streaks probably consisting of thin films of oxide of iron, some black, some brown, included in between the laminæ. Here is a fine piebald specimen—a remarkable piebald specimen—with streaks of brown and black running in all directions, and when you look at it on the surface it shows in certain directions beautiful rainbow colours due to the interference of light at its thin plates. I do not propose to send round these "inclusions" as they might get injured. Here is a great rarity, a piece of mica, which includes not only a number of black specks, which I take to be bits of tourmaline or oxides of iron, but certain red specks which I believe to be garnets interspersed in thin flakes among the laminæ of the mica.

Then another peculiarity which happens is this: that as with amethysts, as even with diamonds sometimes, and with a great many crystals, you frequently find two crystals growing into one another, and forming twins; and they are not always of the same tint or same description, but have their axes in different directions. I have an arc lamp here to enable me to get a more brilliant light, and I will turn that on, and put a ground glass in front of it, so that you will be able

to see clearly the objects which I am about to show you. You can see these things very well if they are held in front of this light, because they have a transparency of their own. Here is the mottled piece, here is the piece with garnets enclosed, and here is the piece with tourmaline included in the centre. Now for the twins. They are, perhaps, not so easy to observe. This crystal, if you notice, appears to be made of two parts, one light and one dark. It really is two crystals which have grown naturally together, and have been built up side by side. An examination with polarised light shows that the axes of the crystals are in different directions. Here is another curious twin, where a corner of one crystal has penetrated into another one, evidenced by a slight change of tint. Here is another one with an obtuse corner of one crystal running into another, and another with an acute corner running into another, visible by reason of a slight difference of the brown tint; and here is a very remarkable one—you may not notice anything from that distance, but this really consists of three crystals, one piece at each end, and another in the middle, as is evident from the colour. If I turn it slightly round you will see that the part which was orange goes red, and the part which was red goes orange. If we had daylight you would see it better, but, unfortunately, daylight is not at our command. You get these singular changes of colour in viewing it from different angles. Now that proves that they are different crystals twinned together.

Then there is the curious and well-known phenomenon of *asterism*—that is to say, the production of stars. To show you the asterism I must have a light of a different kind, instead of having a diffused light I must have a point; and for that purpose I will simply take this little glow lamp and show you the lamp behind some of these pieces of mica, and you will notice at once there are six rays round that point of light, and they revolve as the sheet of mica is turned. Then here is a piece which has eight rays, that is one which has the six with an additional two. I am afraid this cannot be seen very well from a distance, but if you put it to your eyes you can see that one part of the sheet is pale and the other dark, through the light part you see six rays, and through the dark part you can see the other two rays, making eight in all. Here is a piece of 12-rayed mica—a star with 12 arms—and here is a piece which is very uncommon, with six principal rays and other six in between, making it 12-rayed, but with rays not all equal. Here is another piece of 12-rayed mica, and here is a piece of Indian mica, dark red, which shows only two rays which are very faint. I

doubt whether you will see them because it needs a still more brilliant light than we have here; but if you were sufficiently near you would be able to see two rays, one each side, right and left. Well, you see we have two-rayed, six-rayed, eight-rayed, and 12-rayed mica. These asterisms, no doubt, depend upon minute structures, minute crystals which follow the general lines of crystallisation.

Now let us go back to my model (fig. 1), so to speak, my primitive crystal. If you were to take this piece of six-rayed mica, the six rays would be in this position; one pair will go at right angles across the line AB between those two sides which I have referred to as being different from the others; the two other pairs will cross the line at angles of 60 degrees, going angularly towards the corners. The phenomenon of *asterism* has long been known; you know there are certain star-sapphires which will show the same thing, and also certain star-rubies which show the same general phenomenon of six rays. Now there was discovered about 20 or 30 years ago by a certain German professor a very remarkable thing—namely, this. Suppose you take a piece of mica, which must be one you are prepared to sacrifice, and put it down on a fairly flat piece of wood and hit it with a round-ended punch, there will result what he called a "Schlagfigur," —a figure which results from a blow. The kind of pattern you get as the result of hitting mica in that way is different in different kinds of mica. It has something to do with the internal structure of mica. Here I have a very beautiful Schlagfigur, in a sample lent me by Mr. Wiggins. I do not know how it occurred, but the sheet has been hit somehow or another at one point, and there has started a streak in the form of a star. These figures are generally less than a quarter of an inch across, and have to be looked at through a magnifying glass. This one is unusually large. Now if you take either Muscovite or Phlogopite or any kind except Biotite or Lepidolite, and hit it, you get a six-sided star which follows exactly the same lines across the crystal as the star which you see. One pair of arms of the six-sided star, see fig. 9, will lie across between the two straight sides at right angles to the line AB in fig. 1 and fig. 9.

I have not yet got to the end of describing the optical properties, in fact, I have hardly yet begun with the optical properties. I have mentioned quite briefly that some mica is transparent, and other kinds comparatively opaque. That opacity is almost always due to the presence of these additional minerals—either iron, or chromium sometimes, or manganese. I would like now to dispose of the questions

involving the chemistry of them, and so I will first show you an experiment, which was shown to me two days ago by Mr. Wiggins. Here is some Lepidolite from Australia. I do not know whether it is all equally good for the purpose, but suppose we take a piece and split off a little bit. I have two samples which are not equally good, but they are supposed to have come from the same place. This mineral contains soda and lithia; when it is heated in the flame of a Bunsen burner, you will see the yellow tinge which you get with salt—sodium—(for micas all contain sodium more or less)—but if it contains enough lithia you will also see now and then a strikingly beautiful crimson colouration given to the flame; this colouration being characteristic of the metal lithium. If you had a spectroscope to examine the flame you would see the characteristic bright red line of lithium. You see when I hold it in the flame it actually melts. Having got it melted at the top of the flame I bring it down towards the bottom and you observe that fine crimson colour. There is still another thing I want to show you about this same Lepidolite, and for that purpose I must have the room put into darkness by turning down the lights. If you take a piece of mica and split it in the dark you can sometimes see flashes of light emitted, and I am going to split off a bit of this kind of mica in order to show you. You see how it flashes. Now I will take a bit when it is split off, and bend it abruptly in the dark, and when I do that you see it flashing internally. No doubt that is an electrical discharge inside, and the peculiar crimson colouring is, I believe, due to the same circumstance as I have already pointed out—that it contains lithia.

Micas differ greatly from one another in transparency, as you have seen. The transparency of the material depends obviously upon the greater or less amount of absorption of the light by these dark substances that it contains. It very soon becomes opaque if you pile it up thick. If you take a great thick piece like that which I have here and try to see through it, you cannot. Now it is not generally known—I certainly did not know it myself until a few days ago—that mica, though it is, when thick, opaque enough, yet nevertheless is extraordinarily transparent in the edge direction. Now you would naturally think that a material that was like these ordinary specimens, opaque when in slabs 1 in. thick, ought certainly to be quite as opaque if it was an inch thick in the edgewise direction. But Mr. Wiggins showed me an exceedingly beautiful thing the other day, which he has very kindly lent me in order that I may show it to you. Here

is a column up the centre of which runs an iron rod. Upon that rod are threaded a large number of washers of mica of different kinds, pressed down together and then turned in a lathe and polished. I have the means here of lighting up this column from behind, and when you have the light behind this column of mica you will see that it looks almost like a bottle containing oil. Some of the bands are exceedingly black, but some are of amber tint, and some very light. You are looking through 2 ins. of thickness, and yet you see how wonderfully clear they are. In the bottom of the column there are a number of green pieces of mica, and a number of red ones. I do not know what the particular varieties are in that column, from top to bottom, but it is very clear that there are many different varieties. I hardly need say anything more about it.

The "manufactured mica" that is now being used so largely for electrical insulating consists of scraps of mica which are waste for other purposes, but which are stuck together with shellac and heated under pressure in a stove, so as to be very hard. But this manufactured mica, or micanite, is quite uninteresting from the optical point of view. I would like to show you one result of handling mica artificially. Take a piece of ordinary Muscovite and put it into a furnace and heat it. It comes out (showing some burnt mica) looking almost like silver on the surface, very easily flaked off, looking, in fact, almost like silver leaf. Here are the two pieces of the same mica baked and unbaked, that you may see the contrast between them.

Now let me go from this to the genuine optical study of mica. You are all familiar, I believe, with the tourmaline forceps which are used in examining pebble lenses to distinguish them from glass lenses. You know that you have to adjust these two pieces of tourmaline across one another, so as to have the light cut off as much as possible; and then if you wanted to examine your piece of pebble you would put it in between the two tourmalines and look not at a point of light such as this lamp, but at a white cloud or a white surface. Now if you examine any one of those pieces of mica that are clear and transparent by putting it between the tourmaline forceps, you get some very remarkable results. Unfortunately, however, I cannot provide you all with tourmaline forceps, and therefore I must resort to another mode of showing you what is to be seen there. I will adopt the polarising microscope as adapted to the optical lantern; and I will throw upon the screen a number of examples of crystals viewed by convergent polarised light, that is as they would be viewed by eye in these tourmaline forceps.

Briefly, to describe the arrangement, I have an arc lamp in the lantern, with the usual condenser lenses, then a large polarising Nicol prism, and then a system of lenses which will bring the light down highly convergent upon the crystal; then a certain system of lenses to receive that light and render it again nearly parallel, and last of all another Nicol prism to serve as an analyser. The Nicol prisms serve on the large scale as the tourmalines of the forceps do on the small scale. When the Nicols are crossed I have the "dark" field, and when they are parallel the "light" field. And to begin with something familiar I will take a piece of ordinary calc spar, which is a uniaxial crystal, and when examined in this way gives you the familiar black cross and a system of coloured rings (fig. 5), of the same sort as you observe in a

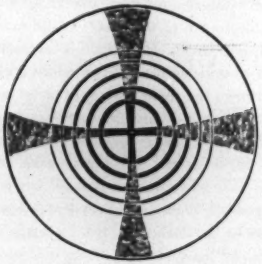

FIG 5.—RINGS AND CROSS IN UNIAXIAL CRYSTAL.

piece of pebble or quartz, with which you are possibly more familiar than with Iceland spar. You will notice that in the case of the quartz the black cross does not go across the middle. In the case of quartz we get a central portion of it which changes colour if one of the prisms be rotated. The piece I have showed you first was a piece of Iceland spar. Now I will take a thinner piece, and you will see that that thinner piece gives me a somewhat larger pattern. I still get the cross but the rings are somewhat larger. Now I take a still thinner piece and you observe that the thinner the piece the larger the ring pattern for one and the same substance. This is Iceland spar and it is a uniaxial crystal, and must be cut so as to be able to look along the axis in order to obtain those rings and that cross. If the piece is not cut quite across the axis, exactly across, the cross will not be in the middle of the field. If you notice in the one case it is rather lower

down in the middle, and as I turn the piece round the cross shifts. That shows that it is not cut quite straight. If you were looking down the axis, the pattern would be absolutely centered in the middle. Now to make us familiar with this let us take two other crystals known to be uniaxial. Let us take a little piece of ruby. It gives you the same thing but on a somewhat larger scale, the substance being less strongly double-refracting. Ruby is less powerfully double-refracting than the calc spar or quartz, and consequently, for an equal thickness, the rings are larger. Here is a piece of jacinth, and with that you see the same sort of thing. Here is a piece of pale emerald, and I wish you to compare that with the ruby. Here is a crystal called brucite, and in that you will observe the same sort of thing, only the rings are on a larger scale. Here is a crystal coming from the Ural Mountains, called a phenakite—practically a white emerald—a gem much rarer than the diamond, but much less worth, and which is frequently palmed off on unwary purchasers as a diamond. It is a white crystal, and it, unlike the diamond, also shows the same system of rings and crosses.

But now if we pass from these uniaxial crystals to those which belong to those other crystalline systems, of which I have spoken, which have axes in different directions—axes of maximum and minimum elasticity—see what the result is. I take a piece of aragonite, and as a result of putting it into the proper position it gives me, as before, a black cross in the dark field, but the black cross has not got a system of rings round the middle of it; in fact you see there are two centres, or "eyes," and if I turn it round a bit I get those two "eyes" separated, and if I go on turning it then the cross forms again. Figs. 6 and 7 show the general appearance. There is really a system of rings round both those "eyes." On continuing to revolve the crystal so that the "eyes" return into the horizontal line, we come back to the black cross again. Each "eye" surrounds an optical axis. Now that is a typical biaxial crystal; but typical with the possibility of there being many great variations. Let us take another biaxial—a piece of common saltpetre. There are the two axes again, you see, but they are much nearer together. If we turn the crystal round, the black cross splits up into these two curved brushes (fig. 7), one passing through each eye. In fact the black cross has parted into two curved portions which re-unite when the crystal has been turned through a right angle. Now I take another biaxial crystal, namely, a piece of baryta. Again you have a black cross, the upright bar of which is a good deal enlarged, and hazy, but having a very much thinner cross

bar right and left. But see how large the "eyes" are in this case, and what wonderful colours we have arranged in rings. When I turn the specimen round the pattern opens out into those two "houppes," or brushes, and with further rotation reforms, breaks up again, and

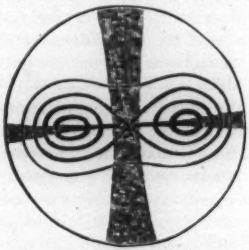

FIG. 6.—CROSS AND CURVES IN BIAXIAL CRYSTAL.

again reforms. You can see all that by personal observation with the tourmaline forceps. If we take any of these biaxial crystals—take a piece of topaz, for instance—it gives the same thing; but in the case of topaz the axes are wider apart. It is a biaxial crystal, with a wider

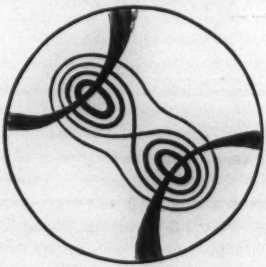

FIG. 7.—CURVES AND BRUSHES IN BIAXIAL CRYSTAL SET OBLIQUELY.

angle between the axes. I turn the crystal round and the pattern opens out and reforms with the black cross which breaks up and reforms again. Here is a crystal of sugar, cut in a particular way across the axes. There is not so much colour, but there are the two

"eyes" characteristic of the biaxial crystal, the two axes being very wide apart. And here is another piece of crystal. This is carbonate of lead. Now this differs completely in detail from the sugar. But both, the sugar and the topaz, have their two axes very wide apart. Now we come to crystals which have the axes very near together, with only a comparatively small angle of 4° or 5° between them; and in this case the two systems of rings melt into one another—you have the two eyes with the two systems of rings round them, but the rings blend into one another and give us a system of ovals (fig. 8). So that the narrow-angled biaxial crystal differs in detail on the one hand from the uniaxial crystal, or on the other from the wide-angled biaxial. Here is a piece of crystal called sanidin, it is of the same biaxial species, but with a moderate angle. You see a system of rings round each axis, and you also see some of the ovals

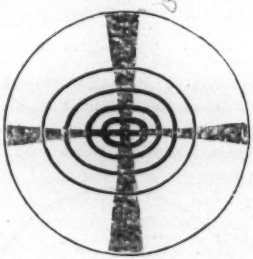

FIG. 8.—CURVES AND CROSS IN BIAXIAL CRYSTAL OF NARROW ANGLE.

embracing both the axes. I take another piece of sanidin. There is the black cross with the two axes, but it is, I suppose, quite obvious that the angle between is small, and we have a system of ovals which embraces both axes. Now I take another crystal, a piece of axinite, in which crystal the angles are so wide apart—nearly 90°—that we can only see one axis at a time; but we can see it is not a uniaxial system, because the rings have got a black streak and not a black cross coming through them. We have the "houppes," but not the "cross." The cross is in these biaxial crystals broken up into two parts—one arm goes through one eye, and the other goes through the other eye; and the second "eye" is not in the present instance within the

field of view. Here is a piece of clear talc (not mica) which is obviously a biaxial, though it looks very like a uniaxial. There is a cross, but the rings are not circular, they are oval, and when we turn the crystal a little bit, the cross splits at the centre, showing that it is clearly a biaxial crystal.

Well, then, we have now learned to discriminate between uniaxial and biaxial crystals. So far, I have not been using mica at all in these experiments. Now let us see what mica will do. Here is a little piece of mica from India, and at once you see it is obviously a biaxial crystal, giving the form characteristic of a fairly wide-angled biaxial crystal, with a great dispersion of colour; and that last feature is characteristic of all the micas. The next piece—I do not know the place of its origin—is a piece of green mica, and again it is a biaxial. But it does not give us so black a cross. Now I take another piece of biaxial mica of a different thickness—it is a somewhat thinner piece and therefore the pattern is larger. The thicker the piece the more concentrated the rings around the two centres of the pattern. Here is another piece of biaxial mica, but look how different it is, with a comparatively small angle, so that we get the ovals instead of either circles or even a regular pair of "eyes." Here is a piece of substance called "uniaxial mica," but it does not give very perfect rings. Turn the crystal round and we see them wobble a little bit, showing that they are really ovals; it is just opening into a biaxial, though on the first appearance it seemed to be purely a uniaxial crystal. I suspect all the so-called uniaxial micas to be really biaxial micas with a very small angle between their axes.

What will happen if you take two pieces of mica and cross them one over the over? I will take two pieces of mica, just ordinary stuff. There is nothing special about the sample; it is just ordinary good Muscovite, rather thin, and therefore you see the pattern to be very large. Take another one, the same sort of thing, split off the same piece. It shows a similar pattern. Now what will happen if I put the two pieces into the polarising apparatus so that they lie across one another? If I set one one way at about 45° and the other at 45° the other way, I get four "eyes" instead of two, and when I rotate the combination I have an oblique cross with the "eyes" at the corners, instead of their being situated on the arms of the cross. These are the curious effects produced by crossing two pieces of mica over each other. I have a little slide here in which two pieces of topaz are similarly crossed over each other. There you see

the black cross with the eyes at the corners, or, in another position, the black cross with the four "eyes," right and left, up and down. The specimen shows exactly the same beautiful pattern in the tourmaline forceps I have here two other samples of mica, which I made from some of these sheets on the table. Here is a piece from Ceylon, of very easily bent mica, which is found to be very nearly uniaxial. I thought it was so at first, but, on examination, it is certainly not. In the present position you see the rings are oval. It is not really a monoaxial crystal. Lastly, here is a piece of that pink-coloured Lepidolite, which is supposed to be monoaxial; but you see again it is not a true monoaxial mica. It possesses a slight tendency to diverge into two axes. So much for the properties of mica when examined by the polarising microscope, or by the tourmaline forceps. To use these tourmaline forceps you want a good cloud or a good white surface, or a lamp with a good opal shade over it. Inserting the crystal, at once you see the black cross, or the houppes. If there is any doubt about the crystal turn it round and see whether the cross splits up into two houppes or not. Now see how this enables you to discriminate between the different kinds of mica. With Muscovite (biaxial), the angles are never less than 55° and never greater than 75°, the angles being measured in the air. Thus you are able to discriminate between different kinds of Muscovite which vary with the amount of potash, or soda, or alumina, in them. Phlogopite, as the table (p. 2) shows, is biaxial. It varies between 7° and 18°. It has water and hydrogen in it, and potash, iron, or magnesia, instead of soda, and that is what makes it dark. With regard to Biotite I do not believe it to be uniaxial; the angle is very nearly 0°, but it may be 1°, or 2°, or 3°, or 4°, or anything up to 6°. Lepidomelane is so black that we do not know anything about its internal structure. Zinnwaldite is a biaxial of 60° or 70° according to the amounts of soda, potash, and so on, in it. With regard to Lepidolite, you will find in all the books a statement that Lepidolite is biaxial, with axes from 76° to 86°, or even nearly up to 90°. Well, I have been examining these samples of Australian Lepidolite. There are certain samples you cannot examine because they are utterly untransparent. Here is a sample—it looks simply a fine grained micaceous rock. But these clear pieces of Lepidolite which I have been able to examine are all narrow-angled biaxial, almost uniaxial. I have not come across one that could be described as having a wide angle of 76° or 86°. The position of the axes is related to the

crystalline structure: for the line joining the two "eyes" is, in all cases, found to lie parallel to the line marked AB in my model, fig. 1., as is also indicated diagrammatically in fig. 9.

Now there is another optical phenomenon of mica that is not generally known. I have known it for a good many years; but when I became aware of it, or whether I found it out myself or was told it, I do not know. At any rate I find it has been known for many years to Mr. Wiggins also, and it is this. Take one of those good biaxial micas of rather dark tint and examine it with the tourmaline forceps or with the polarising microscope. You observe the two axes with a

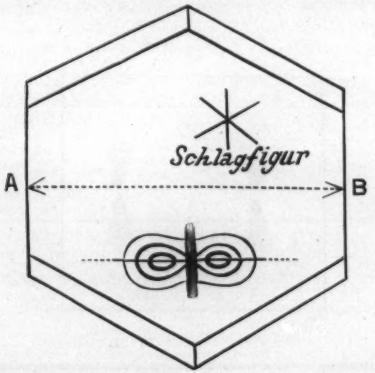

FIG. 9.—RELATIVE POSITION OF OPTICAL AXES IN CRYSTAL OF MICA.

considerable angle between the two. Or take a narrow-angled mica. This is a piece of narrower angle—not so wide an angle as 75°, but more like 20° or 18°. Well, now, there are those two eyes, but you only see those "rings" and "houppes," of course, by examining it in the polariscope. Take away the polarised light and use ordinary light; and what do you see? I remove both the Nicol prisms from my apparatus, but leave the slice of mica between the lenses. I now am using simply a magic lantern and lenses, and nothing more; and I am sending highly convergent light through the mica. Under these circumstances you will see what there is to be seen. It is exactly as though after examining the mica through the tourmaline forceps you should remove the forceps and simply put the mica close to the eye.

You see those brushes, those *houppes*, as we call them. The rings have disappeared, but the houppes are visible against a bright background. Those houppes are where the "eyes" would be, if the light were polarised by tourmalines or Nicol prisms. But you see they are visible without any polarising apparatus at all. All you want is a good white cloud or luminous background. Put the mica close to your eye and tilt it about and look at a good white cloud, and you will observe the very thing that I have shown you on the screen (fig. 10). Now I will show you the finest piece of mica I have ever seen, not the largest, but the finest piece for the particular purpose in question, namely, for showing these houppes. It is a dark piece, you see, with a slight "twin" in it, but it is a wonderfully even piece and of considerable value in money. I place it in front of this lamp shaded

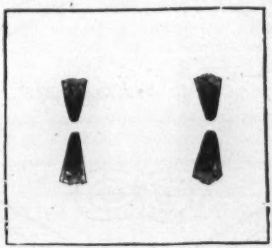

FIG. 10.—HOUPPES SEEN IN BIAXIAL MICA.

with an opal globe. And now, if you are anywhere near, and look in with one eye, you will be able to see those same houppes. You can see it is biaxial for there are two sets of houppes. Now, looking steadily in one direction, you can absolutely measure the angle in between the axes, by finding out how many degrees you have got to tilt the slice in order to shift one or other of the pairs of houppes into the line of sight. This phenomenon of mica does not appear to be mentioned in any book on optics: but I have found that it was known to Brewster, who mentions it in one of his researches. I have known it for some time—I do not know how long. I have a piece of it here, with my handwriting on it, of some years ago. "Indian mica shows the houppes." I knew it, and Mr. Wiggins knew it also. All those pieces that change colour when held in different directions show the same thing. You will see those houppes

quite plainly. There are other minerals, epidote, axinite, and andalusite, which with some variations show these houppes, but there is no substance which is so easily accessible to show them as mica.

Now one result of mica being so delightfully easily split into thin films is this. You can show in the thin films of mica some of the most extraordinary and beautiful effects of physical optics, including those of plane polarised light. I have several combinations here—several combinations made artificially by building up thin mica crossed at regular angles. The first man who did this was Norremberg, and Reusch followed Norremberg. I will show you some of Norremberg's combinations. Take a number of pieces of mica—4, 6, 8, 10 or 12—all cut from the same piece so as to be of equal thickness, and place them on the top of one another, but set with their axes in different directions. First lay one down, then let the next one be turned round through a right angle, the next through two right angles, and so on. If you thus build up the films in regular order you get some very singular patterns. The first example looks, I suppose, very much like two simple pieces crossed. Now the next one shows four that have been crossed in that way, and by so crossing four the pattern is somewhat changed, you see. We are finding that the effect of "eyes" is crushed out at the corners while the combination is developing a system of rings in the middle. Take a larger number of films and cross them, and we obtain, at the centre, more rings, rather square in their shape, crossed by the black cross; but the four eyes are being still further crushed crushed out of existence into the extreme edge. Take a still larger number of films and cross them as described. The eyes disappear altogether, and, instead, you have a system of rings slightly squared at the intermediate corners, not quite truly circular, but constituting a fair imitation in certain positions of uniaxial crystals, though made by building up biaxial crystals across one another, symmetrically round the centre. Then here are some of Reusch's combinations which are equally curious. Look at that one. See how it changes as I move it round, here in the dark field and here in the light field. And here is another one of still greater complexity, made by crossing 12 micas. I tried myself very hard to produce—I mean optically—an artificial quartz. I took 24 micas all of equal thickness, and crossed them at regular angles of 60°, 120°, 180°, and so on, and piled up all the 24 round and round in a kind of spiral one over the

other, by which I hoped to imitate the optical behaviour of an axis-cut slice of quartz crystal. That is the nearest imitation I could get (showing on the screen). I obtain rings, but no black cross. The centre behaves like the centre of an axis-cut quartz, and changes colour, as you see, from red to green, as the analyser is revolved. It possesses the rotatory property of the quartz, although, of course, there is not a particle of quartz in it.

Now much more might be said of interest about these optical properties of mica. I have purposely left out one whole chapter about them—namely, such as you will find in Treatises of Optics, specially in that excellent book by Mr. Lewis Wright, on "Light," wherein he describes the use of thin films of mica in a parallel polarised light for making pretty patterns. Lest you should think I have quite forgotten that there was such a thing, allow me to conclude my demonstrations by simply showing you one or two of these things. Similar slides are to be had of Newton and Company, or of Harvey and Peak, or of many other Opticians who go in for apparatus for experiments on polarised light. Here is a set of pieces of mica of the thickness known as "quarter-wave." They are arranged as 12 sectors. If I take another one which is cut in the form of a ring I get, you will observe, that curious change of colour from light to dark. Or if I take four quadrants of mica, all of the same thickness, but with their axes alternated, and put on this ring of mica over them, I get again that curious interchange of colours. These are all old things. Here is a set of patterns of mica of different thicknesses. If you look at those through parallel polarised light, but not convergent, we get no rings and no black crosses. We get flat tints depending simply on the thickness of the mica film. Here is a pattern sf stripes made of thin bits of mica. If I super-impose upon that a second one like unto it, but slightly displaced, we get a still more complex pattern, which gives us very many changes of colour as it is turned round. I might show you scores of these beautiful objects. But, as these are described in every book on polarised light, I forbear.

In conclusion, I would claim that the substance mica is well worthy of study, both from the mineralogical, the chemical, the crystalline, and the optical point of view. There is a great deal to be learned from it, and I trust you will join with me in thanking Mr. Wiggins, without whom I could not possibly have presented to you so many different specimens as I have been able to do this evening. (Loud applause.)

ON THE NECESSITY FOR REFORMS IN SPECTACLES FOR NEAR WORK—STEREOSCOPIC SPECTACLES.

PAPER BY DR. BERGER.

Read before the Optical Society on January 23rd, 1902.

GENTLEMEN, you know that our common optical instruments have undergone two successive improvements of importance. The first improvement consisted in the substitution of binocular for monocular instruments and the next in the production of stereoscopic apparatus. Three centuries ago the first attempts were made to construct binocular telescopes and microscopes (according to Kepler's system) and it is curious that only much later—namely, in 1827—was the Galilean telescope constructed in a binocular form, constituting the present-day opera-glasses. In just the same way, subsequent to the invention of the ophthalmoscope and othoscope were devised binocular types of the same instruments. As a matter of course, there was no necessity for invention in this matter which we are to consider just now, spectacles being the oldest of binocular instruments; but no attempt had been made to render these latter stereoscopic.

As you are aware, the honour of introducing a new and radical improvement in optical instruments belongs to Helmholtz. By means of the telestereoscope, he proved that a virtual augmentation of the interpupillary distance of the observer leads to a much enhanced delicacy of appreciation of the relief of objects examined. He noticed the same phenomenon in connection with ordinary stereo-photographic work, where the illusion of relief rendered by the two views of an object is the more accentuated the greater the separation of the places whence those two views are photographed. Helmholtz constructed the first stereoscopic telescope, which he described in his treatise on physiologic optics, published in 1867.

In accordance with the theory of Helmholtz have been constructed lately various stereoscopic instruments—compound magnifiers, micro-

scopes and telescopes. The latter are of great practical importance for both sea and land military forces.

I succeeded in 1898 in constructing an appliance to replace the common monocular magnifier of short focus, which is used so extensively in science, industry and the fine arts (by watchmakers, engravers and others), the substitute being a stereoscopic binocular of the same focus. The very great advantages obtained by means of these stereoscopic instruments encouraged the hope that stereoscopic spectacles might call forth an augmented facility of scrutinising objects in their minute details; and this would be of great importance.

The solution of the problem of centering spectacles for near work gave me at the same time a most simple and practical idea as to the construction of stereoscopic spectacles.

What we require is that the principal planes of the lenses of optical instruments should be perpendicular to the axes of those instruments —those axes corresponding to the optical axes of the lenses, and visual lines of the observer.

From this point of view of exactness of centering, the latest improvements in optical instruments with apochromatic lenses have reached a point of culmination which was indeed unexpected.

The human eye itself is but defectively centered; and the principal planes of its different elements are far from being perpendicular to the axis of the eye.

Spectacles for distance can only be centered for those directions of the visual lines of both eyes which are parallel to the median line. Spectacles for reading are centered for convergence of the visual lines at a point situated 33 cm. from the eye.

Thus, as used for distance, spectacles present the correct position of the principal planes—perpendicular to the visual lines.

The desire to employ the same frame for spectacles for distance and for reading is quite wrong. It was observed long ago that persons wearing eyeglasses for reading inclined them more to the vertical the more the observer was required to cast his eyes down. And, indeed, this position is the only one reasonable, as it is admitted that the visual lines traverse the optical centres of the lenses, and should be perpendicular to their principal planes.

For this reason, then, reading spectacles ought to have their lenses inclined to the vertical, with centres lower than would be those of spectacles for distance. The propriety of such inclination of the lenses of reading spectacles is nowadays admitted by everybody.

English and American Opticians have even invented adjustments permitting such inclination, or its suppression, for those cases where the same spectacles are to be employed both for reading and distance.

The lenses of reading spectacles without this inclination are necessarily oblique to the direction of the visual lines, and have besides their spherical an astigmatic action.

This astigmatic action of inclined lenses was known long ago. Thomas Young used it in the year 1801 for the correction of astigmatism in his eye. And I have noticed that persons using an ordinary magnifier often wear it obliquely and correct instinctively in this way the astigmatism of the eye by the astigmatism of the oblique lens.

I have examined the astigmatic action of oblique lenses not only from the point of view of its clinical advantages but also as regards its harmful influence in spectacles as commonly used.

I have proved by experience that the inclination of reading lenses, so that their planes shall be perpendicular to the visual lines, increases the clearness of the object examined, and that the vertical (uninclined) position of such lenses produces an astigmatism which augments the astigmatism of most human eyes.

In view of the fact that the visual lines converge for near distances, spectacles as they are used at the present time are oblique to the visual axes; and according to my personal investigations, this obliquity increases the astigmatism present in the eyes of most persons examined.

The only one rational form of spectacles or eyeglasses for reading is therefore one having a double inclination—having the upper parts at a greater distance from the eyes than the lower parts, and having the temporal edges of the glasses removed further from the eyes than the nasal edges.

An exact centering, that is the coincidence of the visual lines with the axes of the spectacle lenses, is possible for only one direction of the eyes, for which is always chosen (for distance purposes) the horizontal direction, with parallelism of the visual lines to the median line.

The centering for reading is made for a distance of 33 cm. in the median line, the planes of the lenses being slightly inclined. In writing, reading, etc., our visual lines form an angle of about 30° to the horizontal, of which 15° are due to the inclination of the head and 15° to the inclination of the eyes.

Corresponding to every movement of the eye, up or down or later-

ally, a spectacle lens exerts besides its spheric action a prismatic one. The prismatic action is zero in the axis, it is slight in the pericentral part and progressively increases in the outer zones, reaching its maximum at the edge of the lens.

The deviation of the visual lines in lateral movements of the eyes is the same for both eyes in spectacles for distance, and therefore these spectacles do not interfere with co-ordinated movements of the eyes, but only cause trouble in judging the true direction of an object in the peripheric part of the visual field. It is easy to observe this phenomenon in the case of persons who have undergone an operation for cataract. Schmidt-Rimpler remarked that these persons did not estimate distances so well as the one-eyed.

But this author is not right in explaining the phenomenon by lack of the crystalline lens—that is to say, by loss of accommodation. I examined from this point of view persons having undergone an operation of cataract, and I found that their faulty visual orientation was due to different causes ; a prolonged disuse of vision during a development of the cataract, enlargement of the retinal images by convex glasses, adducent prismatic action of wrongly centered lenses, which cause that the aphakic eyes judge objects nearer than they really are. The result is uncertainty of movements, especially when traversing the street. Even vertigo has been experienced by persons using strong glasses who have undergone the operation for cataract (Königstein).

Spectacles for reading cause very often, even quite generally, troubles of the co-ordinative movements of the eyes. We know from the investigations made by Lamare in Dr. Javal's laboratory, that during the reading of a line both eyes execute four or five jerky movements and that both visual lines remain fixed during a certain period (seven-eighths of a second, according to Beaunis) on the same point of the line of printed matter in the interval between each jerked movement. And, in point of fact, a continuous movement of our visual lines would not give us the faculty of recognising characters, since a certain duration of the visual impression of any object is necessary for recognising that object (fig. 1).

You can easily imagine for yourselves the difficulties of co-ordination of the associated movements of the eyes which result during reading with convex glasses that are centered with the centres of the pupils. A line in the ordinary daily newspaper, such as the 'Times,' has a length nearly equal to the interpupillary distance (6 or 7 cm.).

At the beginning of the line, the left eye sees the letters in their right direction, while the images of the same letters are displaced to the temporal side for right eye. In reality, there is an unequal prismatic action of the spectacle lenses for all parts of the line excepting the middle. We can understand thus, for instance, why convex glasses cause actual diplopia in persons who are not accustomed to them.

The troubles caused by spectacles for near work, especially by convex glasses, have been known for a long time, but were, until now,

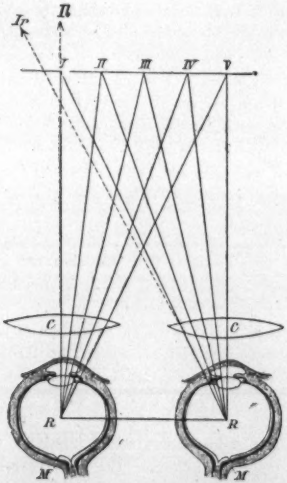

Fig. 1.—*Annoyance of the co-ordinated movements of the eyes in reading a line with the aid of convex glasses.*

explained very badly by ophthalmologists. They were well described by individuals wearing those glasses, saying, "We do see well with these glasses, but we become tired when reading; the letters intermix and dance."

Since the time of Donders it has been admitted that this annoyance is due to a difficulty in the mutual relations between convergence and

accommodation. The hypermetrope, being accustomed to make strong efforts of accommodation, during reading and writing, is annoyed because those efforts are diminished by the use of convex glasses. That this explanation—which we find in the excellent works of Tscherning, Nagel and others—is an erroneous one, is proved by the clinical observation that the same annoyance exists when convex glasses are employed by persons affected with paralysis of the accommodation, as is well described by Schmidt-Rimpler (in cases of diabetes).

We find it stated in treatises of ophthalmology that it is necessary to get accustomed to spectacles for reading, and especially to convex glasses. This means, as I explained, that it is necessary for persons wearing these glasses to learn new co-ordinative movements of the eyes for reading, which correct the faulty concordance of the visual lines at the end of each jerked movement. From daily experience we know that the execution of such new co-ordinated movements is learned slowly and with difficulty; and it happens that persons never reach this attainment in cases when they start at once with very strong convex glasses.

Professor Stellwag recommended the employment of reading spectacles in the first instance only for short periods, with many intervals. He admits, nevertheless, that it is sometimes necessary to combine the convex lens with prisms of 2° or 3°.

In order to avoid difficulty in the co-ordination of those movements of the eyes during reading which are caused by the prismatic actions of the lenses—actions not equal for both eyes—we have to choose glasses having prismatic actions that are nearly equal for both eyes throughout their movements when reading. It is quite understood that it is preferable to choose lenses which diminish the convergence by an abducting prismatic action. The convergence of the visual lines always produces a certain effort of the eye-muscles, while parallelism of the visual lines is the position of ease of the eyes.

In order to diminish the convergence, the following means have been up till now open to our choice.

(1) Professor Brüke's half lenses.

(2) Dr. Liebreich's decentered glasses.

(3) Combinations of spheric lenses with abducting prisms.

(4) Dr. Herrmann Scheffler described under the name of "orthoptic spectacles" a modification of Brüke's lenses. In view of the fact that both eyes form virtually one organ, this author concluded that it

was necessary to give to both of them one large glass, the central part of which he took out. In this way the line of sight of each eye was directed only through the peripheric part of the primitive big glass.

We know now from the works of Dr. Triepel that the most preferable of these systems is Dr. Liebreich's decentered glasses. My own investigations are in absolute accordance with those of Dr. Triepel. All of the other systems give very heavy glasses and even an annoying chromatic aberration which is avoided by employing decentered lenses.

I have calculated the formulæ of the decentering necessary, to avoid the troubles of co-ordinate movements of the eye during reading. And I have much pleasure in showing such spectacles, with an inclination of the planes of the lenses in the vertical (the old inclination) and in the horizontal (the new one).

The scheme which I present to you (fig. 2) explains the mechanism of reading with the new spectacles. You see that the decentered lenses develop from the parts I., II., III., IV., V., of a line for each eye, a virtual image which is further than those lines, with displacement to the temporal side, due to the abducting prismatic action of the lenses, and so much the more so the shorter the foci of these lenses. The visual lines converge at a greater distance than that of the virtual images of parts of the line.

The problem of stereoscopic spectacles is solved by the prismatic action of the decentered lenses. It is evident that the rays which emanate from an object traverse only the nasal parts of Brüke's, Liebreich's and Scheffler's decentered lenses, while on account of the angle of incidence of the rays on the peripheral parts of the lenses these rays are most weakened by reflection just on those parts of the lenses where their prismatic action is strongest.

The inclination which I give to the lenses permits luminous rays emanating from a near object to pass through those parts of the lenses where the prismatic action is strong. Fig. 3 explains why, on account of the strong prismatic action, the difference between the images formed in each eye is very great; wherefore such lenses have a very strong stereoscopic effect, as if a visual organ with artificially enlarged interpupillary distance could examine the object.

You may easily verify for yourselves the stereoscopic action of these glasses by comparing them with the usual spectacles for reading, writing, artistic workmanship, etc. It is easy to prove the superiority

of the stereoscopic lenses. Their distinguishing characteristics are perfect clearness, a distinct relief, and elimination of the weariness induced by convergence.

The far greater convenience of the stereoscopic spectacles over the old ones is noticed at once on comparing strong numbers of the same

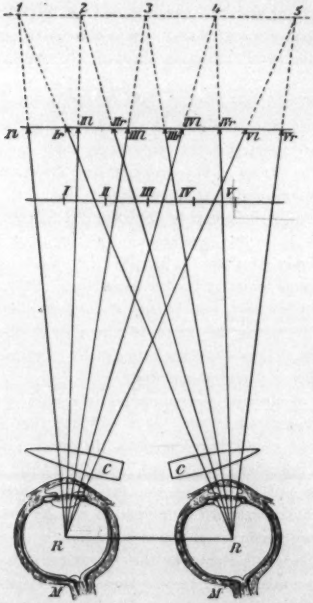

Fig 2.—*Scheme of movements of the eyes in reading with the aid of stereoscopic convex glasses.*

focal lengths—although in weaker glasses the difference is only remarkable after a time.

The theory of the stereoscopic spectacles has been expounded in my name by Professor Lippmann at the Academy of Sciences of Paris,

and by Professor A. König before the Physiological Society of Berlin. The opinions of several authorities about these spectacles are embodied in a treatise published by Messrs. Schleicher Brothers, of Paris.

The practical application of the new spectacles, which I was advised by Professor König to name "stereoscopic spectacles," gave such favourable results that I considered it my duty to inform several most competent authorities on the matter, who encouraged me to proceed with the employment of these spectacles.

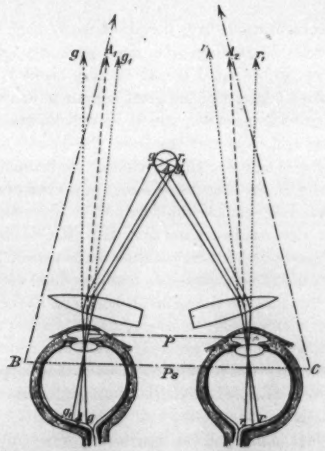

Fig. 3.—*Scheme of the directions of the rays on examining a spherical body by the aid of stereoscopic spectacles.*

Their employment in presbyopia causes, proportionately to the age, a diminution of convergence and a stereoscopic effect which are the more accentuated the shorter the focal lengths of the glasses.

Myopia can be compensated for the diminution of the size of retinal images by the stereoscopic effect of inclined and decentered glasses.

It is important for a myope to be able to correct his eyes for a

shorter distance than 33 cm., without the application of combined concave lenses with abducting prisms. This permits us to give to myopes glasses which do not diminish very much the size of retinal images. The great diminution of convergence which is produced by the stereoscopic lenses for myopes is successfully dealt with by the latter. Convergence is indeed often very difficult for the myope, on account of the elongation of the axis of the eye. I venture to refer as well to the important part which, according to certain authorities, strong convergence has in the pathogeny of myopia; and from this point of view the stereoscopic lenses which diminish convergence seem to be very successful.

My modest investigations have permitted me to reach some improvements in spectacles for reading—the most necessary and commonest of optical instruments—and I would be very happy if the result of the communication I have had the great honour to deliver is to draw your attention to this question, which is very interesting from the point of view of optics as well as from the points of view of the physiology of the human eye and of clinical ophthalmology.

The superiority of the decentered lenses is proved practically. The convexity of plane objects and the deformation of vertical lines were seen also in the stereoscope for the first time after its invention. The application of decentered lenses, with the plane non-inclined, as Professor Liebreich used them, occasions that you lose very much light by reflexion. You observe the same phenomenon if you use spherical lenses combined with prisms; therefore, also for short distances the inclination I showed to you is necessary. Go to a watchmaker in his workroom, which may not be very light, or if it is foggy, and you do not see very much with spherico-prismatic non-inclined glasses. I beg you to yourselves examine the two systems, the old and the new one, and if in your minds you can convince yourselves which of the two systems is the better, that is all I would wish you to do. I thank you very much for the kindness with which you have heard my modest paper. (Loud cheers.)

ON THE SECRET OF GERMAN PROGRESS.

Paper by Mr. H. F. Angus, *Member.*

Read before the Optical Society, January 23rd, 1902.

THE title chosen, "The Secret of German Progress," is one which, I suppose, would attract a great many, did they imagine that there were any real secret, of which something might be learned; but you all know—some very much better than I—that the secret of German success is an open secret, known to all, so well known in fact that it seems almost an impertinence for one who has studied the question only in a very imperfect and junior manner to have anything to say on the subject.

I have ventured to address you, however, because I have a few facts and figures, which have not yet been published, and which may help to swell the reveille which all who have the interests of their country at heart must rejoice to hear sounding on all sides: the few words I have to say may be but the squeak of a penny tin trumpet compared with the bugle note of others, but, if they serve in the very least degree to increase the din and thus awake the sleepers I shall be content. But I would not have you think me one of those weeping Jeremiahs who proclaim with a loud voice that our trade has already gone to the dogs, and who profess to be able to trace the word "Ichabod" scrawled over everything British.

I hold that, with the progress of civilisation, the wants of the world have increased to such an extent that we could not now, were there no other competitors, supply these increased wants as once we did, when Europe's sole industry was agriculture, when the hands of progress were put back on the dial every decade or so by some devastating war; when America, emerging from her colonial days, was only just learning to supply her own people, without thinking of export. I say we could not now supply the wants of the world were these competi-

tors non-existent. As proof, one has only to need workmen. I suppose in every branch of the trade the same difficulty is experienced in obtaining such, as is met with in the branch of which I have intimate knowledge.

I am not now speaking of good men—these, I suppose, will always be difficult to find—but of the difficulty of actually finding men to man the workshops. But having said this, gentlemen, I will admit, and I think every one is prepared to admit, that there is very grave cause indeed for a thorough searching, and a thorough overhauling, of our educational system. I was reading in an educational paper, recently, some remarks on German chemical progress, and at the end of the paper the writer said that we in England here would soon be reduced to the state of those "Happy Islanders"—not Kipling's, but Gilbert and Sullivan's—who existed and subsisted by taking in each other's washing. But, gentlemen, I will put it to you that there is a greater depth even than taking in each other's washing, and that depth, gentlemen, is sounded when you have to put your washing out abroad; and I think myself that, to a very great extent, that has happened in the English optical trade. Export, as far as I am aware, has long since been given up, and now the great majority of our optical goods are imported, and we do not even supply each other's wants, in England here, without thinking of export.

With these few preliminary remarks I will now submit to you some evidence of German progress, which I collected some time back as part of the material for a much more ambitious effort, which, I regret to say, from many causes has not shaped itself into being.

I purpose dealing with statistics compiled from information afforded me by two German firms and one Austrian, Messrs. Zeiss, Leitz, and Reichert respectively, all of whom are well known makers of microscopes, and the first named, of many other optical instruments, including prismatic field glasses, of which, as is well known to you all, they were the originators.

I must say that the figures quoted refer approximately to the end of the year 1899. Taking first the firm of Zeiss, in Jena, 20 years ago they employed 50 men; five years later the number had leaped up to 170, or more than three times as many; in another five years the number had practically been doubled, 327 being the precise number; yet another five years saw the number 580, while to-day (1899) they employ the astonishing number (astonishing, that is, for the class of instruments they manufacture), of 946 men, this grand total being

made up as follows: Theoretical staff, 22; office and despatch, 36; mechanics, 332; Opticians, 371; wood-workers, leather-workers, foundry-men, etc., 129. Of these men, 832 in number, including only those actually at work in the shops, 58, or seven per cent., are foremen, and 178, or 27 per cent., are youths under 18.

Turning now to Leitz, in Wetzlar, who, I may say, manufactures microscopes almost exclusively, we find the same steady progress, if not exhibited in such a striking degree. The numbers employed were —in 1879, 35; in 1884, 100; in 1889, 160; in 1894, 200; and at the present day (1899) 253. This number is divided up as follows: Theoretical staff, four; office and despatch, nine; mechanics, 164; Opticians, 60; case work, etc., 16. The foremen number 10, or 4·2 per cent.; and the boys 18, or 7·25 per cent. of the total number actually employed in the shops, viz., 240.

The firm of Reichert, in Vienna, although smaller, shows an almost identical rate of progress with that of Leitz, the numbers being: Employed in 1879, 20; in 1884, 40; in 1889, 75; in 1894, 100; present day (1899), 150; of these, three form the theoretical staff, eight are employed in the office and despatch department; while of the remainder 120 are mechanics, 30 Opticians, and eight case-makers, etc., the boys being 15 per cent. of the whole. I am afraid the numbers given in detail do not always agree with the totals, but I give them as received.

Here, then, we have a very good idea of German progress in one branch, at any rate, of the optical trade; for a few minutes we will briefly consider the causes, or, to adopt the word used in the title of my paper, "the secret," which has made this phenomenal progress possible.

First, it will be noticed that two of the works under discussion, the two largest, are situated in comparatively small towns, where the rental, not only of the actual workshops but also of the dwellings of the workers, is practically at a minimum.

This naturally leads to a second point—the pay of the worker. As might be anticipated from the foregoing it is less than we pay here in England for similar work, and although throughout Germany there are signs that as trade increases a rapid increase in wages will also take place, yet in 1899 we find a firm of the standing of Zeiss paying their skilled mechanics older than 24 an average salary of £83 per annum (32s. per week), or, if with the firm longer than three years, £88 12s. (34s. per week). This sum includes a bonus, which is given

only in good years, and which in 1897 was five per cent., in 1898, nine per cent., and in 1899, 10 per cent., so that it will be seen that under adverse circumstances the wages bill would fall below the figures quoted; in comparing these figures with those which obtain here, however, it must be mentioned that a week's holiday is given per annum and a pension fund provided.

These two points—low rent and low wages—have no doubt had their influence in the wonderful development of the German firms, but in my opinion only a subsidiary influence. If this were all, the outlook would be brighter than it is, for, in order to compete on equal terms we should only have to remove our works to less expensive sites than the heart of this great city and wait with patience until the wages question had adjusted itself, as I am firmly convinced it will within the next few years; but, and this in my opinion is the crux of the whole matter, the secret, if secret it be, we have not yet considered—the training and organisation which have rendered possible the productions of a Zeiss or a Leitz.

In the most successful of these firms, that of Zeiss, it will be noticed what a arge percentage (27 per cent.) of boys are employed in comparison with the other two—Reichert 15 per cent., Leitz seven and a quarter per cent. It will also be noticed that the percentage (seven per cent.) of foremen is proportionately high.

Herein, to my mind, lies the superiority of the firm of Zeiss over competitors of their own nationality, and much more so over us. I do not wish you to understand that I consider the number of boys employed by a firm an unfailing criterion of efficiency and progress; stated in this bald way the proposition is absurd, but, when we take this fact in conjunction with the well known excellence of the productions of Zeiss (instruments than which no more delicate or difficult of manufacture can be found in the whole range of optics), when, I say, we take these two facts in conjunction, what is to be said of the organisation and system which allows of their co-existence?

I think, therefore, that I may be allowed to say that the number of boys employed by Zeiss demonstrates their superiority, and not only that, but that it gives them a *potential* or *latent* power of progress, if I may use the expression. You all know that there are workmen and workmen, shops and shops, and that a man employed, and usefully employed, in one shop for years might be practically useless in another. Here, then, Zeiss have the advantage. I understand that only under very exceptional circumstances is a man ever taken on from outside.

Practically every man employed has been trained in their own methods, hence the marvellous uniformity of production and finish, evident even in parts which are not, and cannot be, machine-made.

I will premise one or two remarks which I have to make on the system of training adopted, by saying that in Germany, as no doubt you all know, every young man is compelled by law on entering a trade to attend classes for instruction. Such classes the boys employed by Zeiss, of course, attend. A certain number of apprentices are taken who have, in addition, to attend higher classes, and from whom a higher standard of preliminary knowledge is required (that is, they must pass that examination which reduces the term of service in the army to one year). These higher classes are, however, open to the ordinary working boys, if they have sufficient brains to avail themselves of them.

The teaching of optical subjects in the technical school of the town is practically under the firm's control, being subsidized by them, and some, if not all, of the teachers being drawn from the works; half the time spent at this school is during working hours, and is counted the same as attendance at the works.

Such a system of training is, I believe, unknown in this country, at any rate in the optical trade, and although I have detailed the arrangements in force at the Zeiss works, it may be taken as typical (if rather elaborate) of the methods employed by any of our German competitors.

Opposed to this, the best we have to offer are evening classes, where body and mind are more often than not too tired out by the labours of the day to benefit by the instruction.

To return to the Zeiss works, this training of the boys and apprentices, the scientific management of the business, and the experimental work, is supervised by a staff of no less than 18 mathematicians, physicists, and chemists, each of whom holds a University degree; the salaries of these gentlemen, together with the cost of the experimental work undertaken, reach a total of from £6,000 to £10,000 per annum.

Here, then, in my opinion, you have the secret of German progress —a thorough well-grounded *elementary* training of the workmen, controlled and employed by those possessing a *real scientific* training. This is undoubtedly the crucial point of the whole question; that there are other factors in the problem, I, for one, should be the last to deny; there are one or two of very great importance in the branch of the

optical trade in which I am most interested, viz., the manufacture of microscopes, but they would be of interest only to those well acquainted with the microscope, its market and production, and I need not trouble you with them now, but this question of training is common to all the trades in which German competition is felt, and is undoubtedly the question of the hour.

Since the above was written, gentlemen, several events have happened, the most important, perhaps, of which, thanks to Mr. Aitchison, is the formation of an Educational Committee. Of that Educational Committee I have very high hopes indeed, and I can assure the members that when, in three, six, or nine months, as the case may be, or it may be a year, they present their report, that report will be no perfunctory one; but that practically all there is to be known of optical training will have been gathered together, sifted, and embodied in the recommendations that they will bring before you. And yet, gentlemen, its recommendations might shame the judgment of Solomon for wisdom—those recommendations might be backed up by the wealth of Crœsus, and yet salvation might not be found. In discussing several points of the present educational system I was struck by the fact that certain courses, it was stated, would not attract students. Gentlemen, where optical training is being considered, it should not be necessary to have to attract students. (Hear, hear.) That they should be attracted I should be the very first to urge; but for one man who will be attracted there are 99 who will learn because they have to. And as I have just pointed out to you in my paper, in Germany such training does not depend on the pleasure of the students. (Hear, hear.) They are, by law, forced to attend classes, not only for the optical trade, but I understand for practically any trade. When a youth enters a trade he has to attend a technical school; and to give you some idea that there is not, among the workmen in Germany, any very much greater enthusiasm than here, I will mention one fact, which I think is a very telling one. In obtaining this information, and in communicating with these various manufacturers, I put the question to them, as to how many men, having completed their course—their compulsory course—how many men afterward attended technical schools. And, taking the firm of Leitz, who, as we have seen, employ 250 men, I got back an answer which was absolutely startling. It was this—that they had never known any men in their works attend any classes after the compulsory ones. (Laughter.) I think, gentlemen, that is

fairly conclusive. We hear a very great deal about the interest which the Germans, the Americans, anyone but Englishmen, take in their work; but I do think myself that for one man in any country who takes an interest in his work, who lives for his work, there are, as I say, 99 who do not; and it is for these—it is not for the one man—that you have to legislate, because that one man would work under any conditions. It is for the 99 that you have to legislate. And, gentlemen, if we are not in a position to do this, as we are not, and it will be a long time I am afraid before the Government of this country makes technical education compulsory—what I want to put before the members of the Optical Society to-night is this: When these recommendations of the Committee come before the Society is the matter going to be let drop there? I believe it has been suggested—it was at the earlier stages—that certain classes should be subsidized, and a very good thing too. Supposing, gentlemen, the recommendations meet with your approval; supposing that we subsidize certain classes, is that all we can do? Undoubtedly, what we ought to do is this: if the recommendations meet with the approval of the Society then a league ought to be formed—call it what you like, I do not much care for the word "league"—but the members ought to combine themselves together and agree not to employ any youth, whether in a retail shop or in works, unless he is willing to attend classes or to fulfil the recommendations of the Educational Committee. (Hear, hear.) I think, gentlemen, that that would bring to bear a very great influence indeed. It might not in the first instance pay; I do not think it would. The gentlemen who signed such an agreement or joined such a league as that would have to give facilities to the men in their employ, or the youths in their employ, to attend such classes. And if I might suggest, they might also pay the fees; but those fees are really so nominal that they would mean very little either way. (Hear, hear.) But what it would mean to present classes—to the classes established, say, at the Northampton Institute, would be a very great deal indeed. There are a certain number of those classes, and if that could at once spring up to three or four times its number, then optical education would not only benefit, but it could be very greatly improved. Numbers, in all these things, of course, mean efficiency. If you have the numbers to teach then you can command more money—I do not know exactly how it is arranged, but I presume there are Government grants or something of the kind—and with more students to teach, with more money to spend on an efficient

teaching staff, the better are the results. I do not think I have altogether wasted your time, gentlemen, if I leave this with you to-night, as to whether it might not be possible to form such a league by the time the Committee have given in their recommendations. Of course, you could not bind yourselves beforehand because you might find that the Committee had ordered a University education, you know—(laughter)—and that might be a little embarrassing, but I think that most of you who know the members of that Committee know also that the recommendations, whatever they are, will at least be practical. And that is the point, gentlemen, that I want to press home to you to-night. (Loud applause.)

A DISCOURSE ON FILMS.

Paper by Mr. J. T. Sandell.

Read before the Optical Society February 13th, 1902.

PRACTICALLY ever since the discovery of photography a base for the negative image that shall be at the same time infrangible and comparatively imponderable has been a desideratum.

Paper was the first base used in the Talbotype process, the classic invention of Mr. Fox Talbot; this was prepared by sensitising a pure white paper that had been soaked in a solution of iodide of potassium, in the nitrate silver bath.

The method was open to many objections, chief amongst these being the opacity and grain of the paper, and of course the low grade of sensitiveness.

Next waxed paper came into vogue, the grain and opacity was here practically overcome, but the fusibility of the wax at high temperatures, the opaque marks caused by folds coupled with the low degree of sensibility to light obtainable, militated against a long continuance in favour.

We next come to paper coated with a sensitised gelatine emulsion. The dried negative here was at first rendered translucent by soaking in oil or oleoresins, but the operations involved were not at all calculated to confer a long life on the process. Then we find the next step, paper was simply used to support the sensitive film in the camera, and it was so treated with a soluble substratum that after development, washing and drying, the gelatine film negative was stripped off its support. The stripping proved, however, from the uncertainties attending it, to be commercially impracticable.

Celluloid was then adopted, furnishing a transparent, impervious support, which at first sight seems to leave little to be desired; but on further consideration, we find its weaknesses are such that it can only be classed a little ahead of paper. First we have its proneness to chemical decomposition, being a highly nitrogenised body, akin to gun

cotton, which of course owes its explosive properties to the slender nature of the ties which hold its molecules together. These molecules when flying asunder form nitrous and nitric oxides, which react fatally on the latent image or even the developed negative. This, in my opinion, accounts for many of the defective results that are sometimes met with in celluloid films, especially in hot humid climates. Then we have the tendency to curl, which is due to the expansion of its sensitive gelatine film.

Of course the impervious nature of the celluloid, rather than the expansion, is a sure cause of the curling tendency of the films in the developing dish which is present after drying. A change in the hygroscopic and hydrometric condition of the atmosphere after drying will cause a curl. I was of course always conscious of these defects, and was requested, some 18 months ago, to coat celluloid film with three sensitive films. The "Sandell" triple plate was introduced by myself for the obviation of difficulties in photography, with films and glass plates on the transparent basis, of reflection and consequent halation. Well, I coated this thin celluloid film with the triple film of gelatine, and the unctuous nature of the celluloid prevented its adhering. Almost simultaneously, formalin was introduced as a hardening agent for gelatine. It struck me, on finding my thin celluloid base freed from the thick triple film of gelatine, that if I could harden the gelatine film somewhat—toughen it—there was no occasion whatever for a base to support the film. And that being so, it struck me that in the gelatine itself we had a body which would enable us to dispense with the use of a base altogether; and I tried the formalin hardening agent. First of all I may say I tried the ordinary alum hardening bath; but this is quite impracticable from the fact that it has an injurious effect on the latent image. I found that it had a deteriorating effect and slowed the development very considerably, therefore it was quite impracticable. Formalin, however, instead of having a restraining effect on the development and any effect on the latent image, acted rather as an accelerator in the developing, and at the same time toughened and hardened the film to such an extent that it produced the very body I was looking for. Thereupon I proceeded to make the film, and after many experiments I have produced a film which I shall show you the working of to-night. The gelatine used without a support expands equally; but on the support, as I have mentioned just now, it does not expand equally, which is the cause of the curling in the celluloid film. Without a sup-

port the expansion is equal, and the use of this film is attended with no difficulty whatever. In fact, it is rather an advantage in the use of small cameras to find that you have a larger negative than the orginal exposure; but where this expansion is deemed a drawback it is got over by withdrawing the water from the gelatine film, in a bath of alcohol or methylated spirit. This extracts the water from the film and reduces the negative to its original dimensions. And from the nature of gelatine, in drying, we find a support must be used in order to dry the film flat, and produce a satisfactory negative. That means that the film, after developing and washing, must be laid out on a support such as glass, or an impervious support—a pulp or glass or celluloid slab—and dried. After drying the film is stripped and you have a flat negative which does not curl, and remains flat so long as it is kept free from injury by moisture. The fact of the film having a composite nature, that is made of films of varying degrees of sensitiveness, confers on it an enormous degree of latitude in exposure; and the film I shall develop had exposures varying from one second to thirty seconds. These varying exposures are capable of being developed together in the same solution. The great thickness of the film giving the latitude of exposure, also gives latitude in developing—you have more silver there, really, than the light reflected from your object requires to give ordinary density, consequently, the effect of overexposure of the film is simply to give you the maximum development. Supposing we develop the whole action, it gives you more density than you require; so that in developing a spool of varying exposures, you develop for the whole and get the full density. If your overexposures have been grossly excessive, then your negatives will be unprintably dense. This is easily got rid of in the ordinary reducing bath,—ferridcyanide of potassium. The new reducer recommended some time since, ammonium persulphate, is not advisable to use with this film, because of the fact that the ammonia salt annihilates the hardening of formalin. You get rid of the toughening of the film if ammonium persulphate is used. I will pass round samples of the negatives produced by the film, previous to the developing operation. The film, after drying, of course, can be varnished by passing it through the varnishing dish in the same way as, not any other film, but as a paper film. The varnish is a solution of celluloid and the film is varnished on both sides, so that it is perfectly protected from injury. The film is soaked in the formalin bath. This not only hardens it, but wets it and makes it a very soft curious body which has been

compared to tripe. It is altogether strange and different to the developing of any other sensitive film or body you may have seen, inasmuch as the film can be handled quite carelessly—you may throw your spool or bunch of negatives about the room without injuring them. That is altogether a novelty in photographic development. As a rule, you have to be extremely careful to avoid scratches in developing, or melting of the film in holding it in the fingers; but in this case the formalin safeguards you from all dangers on that score. If you are developing in the Tropics, with your temperature 90° or 100°, you need not get ice or any means of cooling your solution. Photography in the Tropics should be quite as easy as in this country by the use of this film. I would like you to pay special attention to the ease with which it can be developed.

I have a complete film here, which is wholly exposed. The film is freed from the spool in the usual way, and rolled up in the hand, and the rolled film is paid into the formalin bath by pulling out a length so, *(indicating)* and pushing it under the liquid, and folding another length until the whole film is immersed. There is no danger of the film sticking one part to another. Owing to its peculiar appearance a stranger is extremely nervous of injuring the film, but with ordinary care there need be no cause for any anxiety on this score. I might have said a good deal more than I have done on the question of roll film photography. All credit must be given to the Eastman Company for bringing to perfection the roll film photography, for seeing from the commencement that a roll film was the easiest and simplest method of preparing a sensitive surface. *(The lecturer proceeded to develop the film he had brought with him.)* I am rolling up the sensitive film in one hand and unrolling the paper in the other. We have marks on the paper binding for cutting out an exposure if thought desirable; but I never do such an enormity, as I call it myself, I always develop the whole roll. The film is fastened to the black paper cover, bound by a piece of rubber plaster. There is the shiny surface and the matt. The matt is the most sensitive, and the one presented to the lens in the camera; the shiny surface is the slower side, being about four times as slow as the matt side. You pay out a length of the film into the dish and push it under the liquid. About two minutes is the time for immersion in this bath, at the temperature of the ordinary living room.

A Member: What is the strength of the formalin bath?

Mr. Sandell: Exactly one per cent. It is a very weak bath and quite innocuous.

MR. WOOLLEY: Are you touching the back?

MR. SANDELL: I really could not tell; it does not trouble one. Putting it in quickly you get, perhaps, a little adhesion, but that need not trouble you, as you can easily pull it asunder.

A MEMBER: Will it stand very hot water after that operation without dissolving?

MR. SANDELL: Oh, yes; if it has had this full dose of hardening your developer may be 150° without injuring the film; in fact, to convince a gentleman from the Tropics, I put a washed negative in a saucepan on the gas, and it did not begin to disintegrate until it got near the boiling point. Both of these sensitive films are made as nearly as possible of the same physical character, consequently there is no tendency to expand more on one side than the other; hence no curling. If it curls it is due to a difference in the physical character of the films, perhaps caused by the extra softening of one film or the use of a softer gelatine.

A MEMBER: Is the expansion considerable?

MR. SANDELL: About 20 per cent., dried with alcohol, and if dried without alcohol, perhaps 25 per cent. The film becomes quite rubber like, and, of course, it may occur to one that distortion is a danger, but there can be no distortion if the film is laid out in the normal way to dry. By that I mean that the film should be sufficiently cold when laid on the support, and the moisture that is left after squeegeeing should be free to move on the support, and then the film takes up its natural shape, and there is no danger whatever of distortion. This is not an extra operation, for the other roll films are recommended to be soaked in water. The only innovation is the introduction of formalin, which is the great consideration. The developer used is pyrocatechin, which leaves perfect immunity from stains or finger marks. There is a good deal of handling necessary in the developing, and a developer which is made up with carbonate has a great tendency to show finger marks; but the pyrocatechin is made up with caustic alkali, and the caustic alkali removes grease from the fingers. When the film is in the dish you get it straight as quickly as you can. Of course, the preliminary wetting facilitates the equal action of the developer over the film. It would not be possible to put this film straight into the developer in this way, you would get unequal development which could not be remedied except by long treatment.

MR. WOOLLEY: Could you cut single pictures out?

MR. SANDELL: Oh, yes; but you see that you can develop 12 varied

exposures in this way with such certainty that it seems unnecessary labour to cut them out; and it is, as a matter of fact, quite unnecessary. Once the film is straightened out it is simply rolled from end to end by the fingers in this way. There is no danger of scratching, the only injury that may happen is from piercing the film with the finger nails, but with a little practice you will keep the finger nails off the film.

A MEMBER: What length of time do you reckon it takes to develop?

MR. SANDELL: That depends to a very large extent on the solution. Short exposures are easily gauged by viewing by transmitted light, or even a correct exposure, so that you can judge the density. It is only with the grossly over-exposed that it is impossible to judge the density; then you have to go on in the dark. The only trouble that may ensue then is the necessity for after reduction. With these films the normal solution gets at the back, but with a plate or film on an impervious base the normal solution may be said to never arrive at the back. It is acting on both sides here, so that your light action which is passed through the film very quickly enables you to be positive of the value of the exposure given. The pyrocatechin solution here is diluted about one in six.

MR. WOOLLEY: Do you find that the most suitable developer?

MR. SANDELL: Everything considered, I find it the best; but hydroquinone and metol, for some reason or other, are not satisfactory. They fog so quickly. There is no staining with the pyrocatechin, and there is nothing whatever against it.

A MEMBER: Would it be possible to use a formalin hardened gelatine as a support for an ordinary emulsion for making prints by the three colour process and developing them in warm water?

MR. SANDELL: Using gelatine as a base, the difficulty would be, of course, unless the film were made in the same way as these films, the difficulty would be in coating it.

The MEMBER: Yes, but you could do it commercially, perhaps, by hardening the gelatine first and using it as a base instead of the celluloid film.

MR. SANDELL: Not very well; if you harden the gelatine previous to melting you weaken it to such a point that it is scarcely possible to coat it. You can mix the formalin with the developer if you wish. It seems to have no ill-effect.

MR. WOOLLEY: Not with the sensitive emulsion?

MR. SANDELL: Oh, yes.

MR. WOOLLEY: You do not find chrome alum answer?

MR. SANDELL: Not for hardening it. It slows the film to such an extent. For fixing you want a deep vessel so that you can put the film in and let it swim easily about, stirring with a stick or your hand. Of course, the fixing bath may be warmed up very considerably, and the film can then be fixed in about a minute, the fixing taking place on both sides.

MR. SALT: What is the length of the film?

MR. SANDELL: It is almost 6 ft. The length of a half-plate film, when dried without alcohol, is about 7 ft. Squeegeeing may be dispensed with if you use the alcohol strong enough. Cut the films and put them flat in the alcohol dish. They dry then, practically, in the alcohol, and you get rid of the slight amount of moisture left by placing the films between fluffless blotting paper. I have heard several who use the film say they never squeegee. Squeegeeing is, however, an operation that most Photographers practice sooner or later.

MR. WOOLLEY: Perhaps that is done, because it makes the films lie flatter.

MR. SANDELL: Well, you can make them very flat in the spirit bath. This (producing same) is a pulp slab for squeegeeing, in which operation it is advisable to put the negative side—the printing side—upwards, and that is easily seen by the colour of the deposit. The negative side is where the amount of the reduced silver is in excess, and is the blacker. If you get the film in a suitable light you can easily see it. There is a danger in warm weather of the film sticking if put down the reverse way. The gelatine is so much diluted with the silver.

A MEMBER: How long does it take to dry?

MR. SANDELL: If in a favourable position, they dry in about 10 hours without the spirit bath; but as a matter of fact I always give the film a short immersion in the spirit bath which facilitates the drying.

MR. WOOLLEY: Now that it is hardened could you not dry it in front of the fire?

MR. SANDELL: No, you get currents of air there. It would contract irregularly, and it is better not to force the drying. After the spirit bath, especially if a little glycerine is added to it, the film may be stood on the warm mantel; but if put in front of the fire, the draught which would get to it, would be against it. That finishes the demonstration, Mr. President.

The PRESIDENT: It is my pleasing duty, gentlemen, to congratulate Mr. Sandell on the very instructive paper, and the interesting demonstration, with which he has favoured us this evening, and at the same time to propose that we pass a very hearty vote of thanks to him for having devoted this evening to us. One of the most delightful features of our optical science is, that if we come to hear a paper we can never be quite sure what particular branch of our science we are going to be instructed in. The subject of photographic films is one, perhaps, of the most interesting of what one might call the by-ways of our science. Speaking personally, I have very little technical knowledge of the photographic side of optics. There are many in the room who are much better able to appreciate the technicalities of Mr. Sandell's paper than I am; but we have all, I am sure, admired the dexterity and facility attending his demonstrations, and as a mild kind of amateur Photographer, which is all I can claim to be—we are all amateur Photographers nowadays—the demonstration has been of the greatest interest to me, and I may also say of some service. I have great pleasure, gentlemen, in proposing a hearty vote of thanks to Mr. Sandell for his paper. (Cheers.)

MR. ARONSON seconded, and the vote was carried by acclamation.

The PRESIDENT: Has anybody any questions to ask Mr. Sandell? I am sure he will be very pleased to reply to any questions which may be put to him.

MR. WOOLLEY: I was going to ask Mr. Sandell about the price of the film?

MR. SANDELL: You have gone a step in the right direction, as these films are about 25 per cent. less in price than the celluloid films.

MR. WOOLLEY: They seem to be keeping up the price of celluloid films, and they do not cost so much as plates to manufacture.

MR. SANDELL: There are points involved in the manufacture of these lengths of films. The same as with large plates, you must make them free from defects. You have to reject very much more than you have to with plates. Cut films much more resemble the plates in price than the greater lengths. I do not think you will ever find celluloid films as low in price as plates. There is more labour involved, of course, in spooling them, and I do not think they will ever get down to the price of plates.

MR. WOOLLEY: I believe they have to be pickled a week before being coated.

MR. SANDELL: That is not the case with the film that holds the market.

MR. WOOLLEY: The Eastman film?

MR. SANDELL: The Kodak film—the base of that is prepared just previous to coating. It is said the liquid celluloid is poured on to the glass table on which the sensitive emulsion is always coated.

MR. WOOLLEY: That is one of the Kodak patents?

MR. SANDELL: Yes, they have a patent on the glass table.

MR. WOOLLEY: And nobody else can get celluloid as cheap as they can. That was the great drawback in respect of not being able to compete in that manufacture.

MR. SANDELL: I do not know that anyone would care to make them in that way. I should not myself, because the great drawback with celluloid is its use while it is in the green condition, as the makers call it. I do not know how they get over that difficulty. It seems to me to be the cause of many defects. The other makers of celluloid films coat the sensitive emulsion on ready prepared celluloid film in long lengths, and this film is prepared and ripened in kilns, and the makers contend that many possible causes of defects are removed in the treatment. Where the film is made in the morning and coated in the afternoon, treatment thus is quite out of the question. So that it seems to me—I do not wish to say anything prejudicial to another film—it seems to me that this must be a source of trouble.

MR. WOOLLEY. Are you ever troubled with electric discharge when you take the films off the tables?

MR. SANDELL: Oh, yes, but not comparable with the discharge in the case of celluloid.

MR. WOOLLEY: That spoils a good many films, I believe?

MR. SANDELL: The extraordinary marking—lightning-like markings—which you sometimes see over the films, is due to electric discharge; but with gelatine, it is impossible to remove the fully dried film from the glass. It has to be damped somewhat, which lessens the discharge. It is a mere trace.

MR. WOOLLEY: Do you get many spoilt films?

MR. SANDELL: I have never seen one. You can just see a slight, very faint luminosity. But as I say, if the film is in good condition to remove from the glass you cannot see it.

MR. WOOLLEY: What is the best way of judging as to its condition for removal?

MR. SANDELL: By lifting one corner of the film. If it is brittle

and breaks, leave it alone until it has taken up a certain amount of moisture.

The PRESIDENT: You coat a big stretch, as big as this table, and then cut it up?

MR. SANDELL: We are limited to the length of plate glass that a man can carry. This is about 6 ft. long by 18½ ins. wide. It is carried into the drying room in these lengths; and unless I were attempting to cope with the cinematograph trade I should not attempt anything longer. I should then have to adopt another plan.

The PRESIDENT: Mr. Sandell, I have pleasure in asking your acceptance of the very hearty vote of thanks which has been accorded to you for your paper this evening.

MR. SANDELL: I am extremely obliged to you, Mr. President and gentlemen, for your kind reception this evening of my weak efforts at a paper. I am a poor lecturer I am afraid, and I have very little time at my disposal for getting up an elaborate paper. I have depended, of course, on demonstrating the easy working of the film as the greatest novelty, and I hope my efforts have proved this. I thank you very much for your kind vote of thanks.

PRESIDENTIAL ADDRESS TO THE OPTICAL SOCIETY.

April 24th, 1902.

By Mr. Charles Hyatt-Woolf, F.R.P.S.

IT is with real diffidence that I undertake the duty imposed upon me by the position to which you have done me the honour to elect me. Not indeed that I could be supposed, in view of my professional experience, to be ignorant of those subjects on which a presidential address to your Society must touch; but I am impressed with the difficulty, and indeed impossibility, of entering on the discussion of some of these subjects so deeply and fully as my talented predecessors in office. The time at my disposal, and the ability at my command, will not allow me to follow Mr. Thornthwaite into the early history of the development of optics and the philosophy of sight; nor Dr. Walmsley into the technicalities of instruments of precision applied to military and other purposes; nor—unless very cursorily indeed—Mr. Dixey, into the highly important questions that arise in connection with the education of the Optician. Mine must be a rather more superficial rôle; and I propose to inflict on you, in the short space of time for which I shall trespass on your patience, some thoughts to which I have been led in the exercise of my profession, with respect to the organisation of the Trade generally, and to the present position in the community, and future prospects, of Opticians.

The development of the manufacturing, and the wholesale and retail, branches of that trade with which we are all so closely concerned, is taking place along several very distinct lines; and the distinctions I speak of are accentuated by marked differences of opinion obtaining between some of us as to the conditions under which certain schemes, intended to promote the honour and dignity and prosperity of the trade, should be organised and administered. The forceful manner in which some such matters of opinion have been expressed will be quite within the recollection of most members of the Optical Society; and the apparent difficulties which still stand in the way of

a conciliatory settlement of all pending questions will doubtless be present to the minds of many of my hearers. Personally, I think that some of these said difficulties with which we are confronted are much more apparent than real; and I believe that with the progress of time, by means of the free and friendly discussions which take place so frequently at the meetings of our Society, and of other societies with kindred aims, a rapprochment will be brought about between those who at the present time stand most widely apart. I foresee of course that we must adopt the most conciliatory manner, and invoke methods of compromise, with reference to the somewhat difficult problems we have to solve; but on the other hand, I myself regret nothing of the outspoken language which has been used by some of our members in this connection; for there is no doubt that, in some quarters, purely personal prejudices, or perhaps the dictates of self-interest, have had results which those affected by them—although really animated by some of the best intentions, and with a genuine desire for the welfare of the Trade—were unable to realise in their true bearings. It is a most difficult thing for any one of us to divorce his judgment in such matters as these from the influence of prejudice and aims personal to himself and to that group with which he is most immediately associated. And I think that, it may be, the drastic "surgical operations" to which the views of some gentlemen have been subjected at certain meetings in this room, afforded the only hope of removing a veil most detrimental to their usefulness among us. I feel that, for tearing down the hood which self-interest and what has been termed self-centration is apt to put before our eyes, there is nothing else equal to a bit of thorough-going criticism in public—such criticism as has been so vigorously administered from time to time in this room. Therefore I say again that I do not regret the hard words which have been used on some of these topics. Their purpose has been served; and so far as the future is concerned, I trust that we of the Optical Society will have the good feeling which will enable us to forget them.

For my own part, I am free to say that I do hold very clear views on most, if not all, of the controversial subjects with whose discussion our Society and kindred associations have been concerned during the past few years; but I feel that the professional position I have the honour to hold, and not less the honorary position for which I am indebted to your kindness this evening, forbid me to obtrude these views of my own so fully and so insistently as I would perhaps like.

It shall be my endeavour now, at any rate, to bring before you a little sketch of the rise and progress of education, of organisation and of prosperity in our optical trade, without either partiality or prejudice with respect to the various agencies concerned.

There is one difficulty in which I find myself, and of which I seek to dispose at the outset. It is that, without touching on my own business, which I am desirous to avoid on this occasion, I am somewhat handicapped in the matter of reference to historical detail. I will just permit myself to say that the organisation and certification of Opticians has been the subject of constant discussion and, if I may use the word, exhortation in the columns of the newspaper I am connected with, since its inception at about this date 11 years ago. In the very first volume of the newspaper I refer to, measures for the certification of Opticians were urged, and the foundation of an institute (based on the lines of the Pharmaceutical Society) was advocated.

Now at the time I have spoken of, 11 years ago, the volume of trade and the class of trade conducted by Opticians on an average was something vastly different from what it is to-day. Referring particularly to the spectacle industry, the quantity of goods sold was much smaller than at the present time, and it is to be feared that among the glasses then adapted for the correction of defects of vision there was a much larger number of misfits. The process that has been going on during the past decade or so, and is responsible for the immensely improved conditions of the trade at present, is a process of education and, as I said before, of the co-operation of many persons for the collective good of all.

Speaking as I do before the Optical Society, it is natural and proper that I should refer in the first place to the origin and progress of our own particular work. The advantages of association for the purpose to which I alluded just now were very clearly and insistently brought before the attention of the trade; and it was, I think, on the 27th of April, 1899, that an informal meeting of some half a dozen gentlemen —Messrs. Aitchison, Cocks, Willmott, Purser, Sanderson, Grenier and Salt—constituted itself as a committee, having for its object the formation of an Optical Society to promote intercourse between all sections of the optical trade—particularly from a social point of view —and to work with and not against bodies at that time existent which were particularly devoted to the settlement of educational and business questions. On the 28th day of the same month, Mr. Thorn-

thwaite, who was at that time Master of the Spectacle Makers Company, quite independently proposed at a meeting of the late Optical Committee that the formation of an Optical Society on lines which had been suggested in THE OPTICIAN ought to be considered. Clearly the time had become ripe for the formation of a society such as this which I have the honour to address; but to the seven gentlemen I first mentioned must be accorded the credit of having initiated the scheme and, by their energy and enthusiasm, carried it forward to a triumphant realisation. If without drawing invidious distinction I might be allowed to particularise two only of the founders of the Optical Society, I would name one—Mr. William Salt—whose unselfish devotion during the past three years has done more than any one other agency, and perhaps more than the work of all the rest of us put together, to promote the development and welfare and honour of the Society. I would name also Mr. Aitchison, to whom the Society is indebted for support of every material character, and whose initiative we have to thank for some of the most important departures we have thus far made.

I think I may say that, since the time of which I have just spoken, the progress made by our Society, its growth and its activity, have received no check. What we have accomplished by way of promoting concord and amity among the members of the trade has been most valuable in its effects, both directly and indirectly. Some years ago, it might be said of social intercourse in the general body of the trade, that such did not exist. The members of the trade were divided among little cliques, debarred from effective intercourse with one another by something I am afraid very much like jealousy and distrust. At that time (we all know of the aphorism concerning a house divided against itself) there was practically no "back bone" to the optical trade; by which I mean to say that had the general interests of the trade been attacked from any quarter, the organisation of an effective resistance would have been difficult. There was indeed, as it happens, the means available for intercommunication among the trade; but there was wanting that feeling of confidence and camaraderie so needful to an agreement on joint action where petty differences of interest and inclination have to be submerged. How fully this want has been supplied, and with what promise of still further benefit in future, I need not say.

What are the chief things that we have accomplished thus far? First and foremost I would put (I have already put) our social

organisation. The conversaziones held by this Society are functions in every way quite remarkably successful, considering the yet short duration of our corporate life. The friendly intercourse that we enjoy at these social meetings must, in very many cases indeed, be attended with mutual benefit commercially as well as otherwise. The exhibitions of instruments that we have had, I venture to characterise as among the most valuable features of these entertainments; and I hope that in future such exhibitions and demonstrations may be made a more regular and prominent feature of our social gatherings than has sometimes been the case. We cannot do better than follow the lead of the Royal Society in this matter, and it seems to me that, besides welcoming the exhibits of absolutely new inventions (which of course the organisers of our social meetings will always be glad to do) we must derive many advantages from the opportunity of examining the various types of ophthalmological instruments, under conditions in which there is no obligation whatever to decide whether or not any particular instrument is one that it is desirable to acquire for one's personal use. It must always be remembered that an exhibition of ophthalmological instruments is one of a highly specialised character, and not likely to appeal much to the public at large; so I believe that, if ever an important and thoroughly representative exhibition of such instruments is got together in London, it must be (as I think Dr. Walmsley before me suggested) under the auspices of the Optical Society, whose authority and influence would alone be adequate to ensure success. In the meantime, I am sure that we would all welcome demonstrations at our meetings with the various types of sight-testing appliances—just as we should welcome the accession to our library of a representative collection of trade catalogues.

This brings me to another phase of the work of this Society—that of the collection and collation of trade literature for the benefit of our members. I am sure that the thanks of all of us are due to Mr. Sutcliffe, our present librarian, for the valuable work that he is doing in this department. And at the same time, if our gratitude may take the classic form of "a lively expectation of favours to come," I would express the hope that, in accordance with suggestions which have been made from time to time, we may find in the near future that the lending department of our library is in full operation, to the benefit of many, and especially of country members.

This mention of our friends in the provinces leads me to express

the strong sympathy that I have, and which we all feel I am sure, with proposals for closer association of our Society with the several bodies now working independently of us, but with like aims. It seems to me that a close affiliation of these local societies with our London organisation would be of enormous benefit; and I am glad to know that the Council has already given this matter some attention.

The subject of standards is one to whose importance we are all alive; and I am not tempted to say more upon the matter here than that I trust no conclusions will be arrived at by any of the sub-committees charged to make recommendations on this point, without the very fullest consideration. And I hope, I am sure together with all present here, that when standards of calibration are eventually formulated by our Society they will be accepted and adopted by Opticians generally, not only in this country but, I venture to say, throughout the world.

Another direction in which our Society is doing useful work is in the promotion and development of scientific knowledge, applied in connection with the Optician's craft. Many scientific societies exist which profess this sort of thing for their sole end and aim. In our case, I am glad to say that we have other objects in view besides the cultivation of pure science; yet I think we may well pride ourselves on the high character of many of the technical papers which have been contributed to our proceedings, and on the scientific eminence of some of those lecturers we have been privileged to hear. I may say without compliment or exaggeration, that several contributions to the proceedings of this Society deserve to take rank with what is best in scientific literature.

It is a natural transition from the mention of original research and invention, and methods of work in the factory or testing room, communicated through our Society, to pass to the discussion of education in a broader sense—as applied in the process of "qualifying" among those of us who have already attained to man's estate, or else to preparing our youthful friends of the coming generation for those duties to which they are expected to devote themselves a little later in life. This is indeed a big question. And here again the Optical Society is fortunate in having organised a most energetic and efficient committee, to take the whole subject under consideration for the purpose of elaborate report. It is to the initiative of Mr. Aitchison that we are indebted for the constitution of this Committee; but it is very largely

to the activity of Mr. Angus, the Hon. Secretary, that we owe the gratifying progress which has been made thus far in the elaboration of that general scheme of studies which it is our object to evolve. The subject naturally divides itself, broadly speaking, under three heads. First the education of the workman, second that of the young Optician just apprenticed or about to be apprenticed to our trade, third that of the man already engaged in the business, as a merchant handling optical, photographic, electrical, nautical, philosophical and mathematical instruments, as a photographic Chemist often called on to advise his amateur customers respecting the performance of their dark room work, and last, although by no means least, as a Refractionist on whose advice members of the public have to rely for the correction of their mechanical defects of vision, and to whose hands the execution of Oculists' prescriptions is entrusted.

For the education of our workmen there can be no doubt that the workshop is the proper place—it is the educational institution *par excellence :* but the extent to which it is employed for this purpose of turning apprentices into workmen having a competent all-round knowledge of their trade is, naturally, a matter of private contract between master and man ; it is a matter of arrangement in which the employer is compelled to act with due regard for his own interests, present and prospective ; and it is unhappily quite true that, under existing conditions in the manufacturing industries, circumstances often necessitate that workmen should be occupied, from the commencement until, perhaps, the end of their industrial career, in a very limited range of manual operations. Here then, in view of this diminution in the educational facilities of our workshops, brought about by the introduction of complex machinery and the sub-division of labour in its use, we can recognise the enormous utility of such places of manual learning as is the Northampton Institute, presided over so ably by our past President, Dr. Walmsley. The technical classes held there provide a substitute for such variety in workshop experience as is not now to be gained by young men whose parents are unable to afford a substantial premium for the privilege desired. There is no doubt that, simply by bringing under the attention of the Trade those special advantages which the Northampton Institute and some other institutes of the same kind offer for the instruction of young instrument makers, our Educational Committee will do a valuable work.

When the intention is that a young man should apply himself not merely to mechanical work (a certain amount of mechanical work

is called for even in the retail shop, if small repairs are to be done, and slight adjustments in spectacle frames are to be executed, as they should be), but if in addition to this mechanical work the beginner intends to devote himself to the professional occupation of testing eyesight for the correction of its non-pathological defects, then a broader and higher education is, of course, called for. The professional Refractionist ought to be a man of refinement and of general culture, so that he may impress his customers with a feeling of confidence in his ability. The public have to judge by superficial evidence, of course, as they know nothing of the technicalities of our science; and it is certain that they will be inclined to accord most credit for professional ability, in those quarters where the possession of a good general education is apparent. No doubt these considerations will have due weight with our Committee as they formulate their scheme for the preparation of young men intending to adopt the career of an Optician.

Happily the advantages of modernised technical education are not confined by any means to the coming generation. That "it is never too late to learn" is true in the most literal sense of the words; and the movement that has been on foot for some years past, having for its purpose to bring about a higher standard of knowledge and of skill among those established in the industry, has been attended with results of a most gratifying character. The impulse to this higher technical education, in the matter of sight testing and so forth, has come from within the body of the Trade itself; although as a reflection, perhaps, of the tendency that has manifested itself so strongly in trade journalism during the past decade. Some of our leading Opticians—I do not take the liberty to mention names here—have made a close study of the optical characteristics of the human eye for very many years past; but others there are, as I say, whose interest in the more recondite branches of the art of sight testing has only been awakened in comparatively recent times. Many of these latter must feel a real debt of gratitude towards our friends, Mr. Lionel Laurance, Mr. Sutcliffe and others, who have devoted themselves to the exposition of the science of visual optics as it is involved in the business of a retail Optician. I am sure if we have regard to some of the papers that have been read before the optical societies in this country, by men who were avowedly quite ignorant of the subject of refraction up till the recent period I speak of, we must admit that the progress made in these studies among the older members of our industry has been

phenomenally rapid, and most creditable both to our teachers of visual optics and to the students themselves. A special faculty is called for in the work of education when undertaken with reference to matured minds, long unaccustomed to the reception of knowledge from the mere school teacher or lecturer; and I think that in this particular respect the members of our trade in this country have been most fortunate at a critical juncture in the development of events.

This brings me to rather a difficult and thorny part of the discussion which I am inflicting on you. It is not my intention at this moment to detail those qualifications which I have been led to think should be possessed by a thoroughly competent Refractionist. But what I have said respecting education naturally brings under review such work in this direction as has been done, encouraged or aimed at, by the Worshipful Company of Spectacle Makers. Since trade journalism was established in our industry, I feel that I may justly claim that the requirements of the trade were strongly and pointedly urged on the attention of the Spectacle Makers Company; and these requirements, expressed through the medium I have indicated, were promptly and fully recognised by Mr. Past-Master Thornthwaite, without whose aid it is unlikely that any such important undertaking as is the scheme of the Company would have been brought to fruition. We are faced with the facts that, despite the revolution—I can call it nothing less than a revolution—that has taken place in the conduct of the retail optical industry, and in which the acts of the Spectacle Makers Company have borne an important part, there is still considerable disaffection towards the Company evidenced among our members, whom we are bound to regard as representative of others in the trade at large. What is the cause of this? I feel bound to put to myself and to bring before you this question, for the reason that I have stated before, viz., that I think there is nothing to be lost by any of us by reason of an outspoken "political" discussion, and there is everything to be gained by frankness and publicity. I certainly think, with all deference to the earnest work of Mr. Thornthwaite and to the zeal of Colonel Sewell, that the labours of the Consultative Committee had everything to do with the solution of those difficulties which confronted the founders of the scheme at its inauguration. And I can understand the feeling of our friend, Mr. Aitchison, that, by declining the further aid of the Consultative Committee, the Spectacle Makers Company (acting, indeed, well within their rights) did in a considerable degree divorce themselves from that intimate connection with the

trade which should be a chief source of their strength and influence.

On the question of the Official Instructorship too, we are compelled to formulate and to state some opinion, if we wish to meet and to weigh all the facts of the case in a straightforward manner. And I must say that it is my own personal opinion that our able friend, who holds that post, had been well able to maintain his position among us without it. I do not agree with those who say or hint that there has been any impropriety in the appointment of our esteemed friend as Official Instructor. I have carefully looked into the matter, and collated the information I have been able to obtain from many and various sources, and as a result I feel able to say most emphatically that, of all men, Mr. Laurance is "the man" for the position. But, then, why this position, under the circumstances obtaining just at present? And if it is not absolutely necessary that an Official Instructor should represent the Spectacle Makers Company among us, why should the feelings on this matter of prominent members of our industry and liverymen of the Company be pointedly ignored? It is not my place as an individual, and certainly not my intention as President of this Society, either to endorse the propriety of the question put or to suggest an answer to it. I will just say this, to illustrate the attitude of mind which I, myself, adopt with regard to the matter at issue: I consider that if any serious measures were taken by the Company to further the optical education of Refractionists, then the appointment of so able a teacher as Mr. Lionel Laurance would be well warranted if not, indeed, essential. I do not, myself, admit anything in the nature of backstairs methods or underhand proceedings in the conduct of the S.M.C. affairs; and I am convinced that those who suspect something of that kind to exist are utterly mistaken. No one who knows Colonel Sewell personally, as I have the pleasure of doing, can imagine that a gentleman so honourable would lend himself to anything of that sort; nor would anyone who lays claim to the friendship of Mr. Lionel Laurance suppose for a moment that he would seek or exercise any undue influence on the conduct of the Company's affairs. But the Spectacle Makers Company, like Cæsar's wife, should be above suspicion; and it is just for that reason, I surmise, that some of our members think the Company would have done well, even while retaining, perhaps, the aid of their Official Instructor, to have retained also those advantages in the way of opinion and counsel and other assistance which were theirs so long as the Consultative Optical Trade Committe remained in office.

There is another very important undertaking which is directly the outcome of suggestions put forward in the manner I have already indicated. The British Optical Association had its origin just like the Certification Scheme of the Spectacle Makers Company, in the journalistic discussions to which I have already referred. The great fault, if I may be allowed to say it, in the proceedings of the British Optical Association at a very early stage in its career, was that the formation of the Society was unattended with such full and free publicity in the trade as had already proved its usefulness, by bringing to a focus that co-operative zeal which resulted in the organisation of the B.O.A. itself. There was lacking such a representative character, and such a reliance on the popular vote, as would have ensured to the B.O.A. almost unanimous support throughout the industry, and would have disarmed a good deal of criticism which at that time found expression, to the detriment in some degree, perhaps, of the prospects of the Association. The bureaucratic constitution that was at one time attributed to the Council of the B.O.A. is now a thing of the past; and while not so fully representative of all classes as is the Optical Society, we may now claim for the B.O.A. that it speaks with authority upon behalf of a very large and important section of the retail trade—on behalf of practically all our most advanced Refractionists. And one vital advantage that is possessed by the B.O.A. over the present examining body of the Spectacle Makers Company—an advantage on which it is hardly possible to lay too much stress—is that sight testing is clearly recognised as a part, and the most important part, of the curriculum in which the proficiency of students is to be tested and certified by examination.

There is no question in my mind that it is an unfortunate circumstance for the trade that the Worshipful Company of Spectacle Makers have not yet seen their way to act more independently—with less regard for the susceptibilities of the medical profession. Not that I would suggest for a moment that any Optician, any member of our Society or of the associations with which we are allied, would wish to encroach on that province which is legitimately restricted to medical men. Anything that savours of quackery is an abomination to me, and I feel sure to any and every self-respecting member of the optical industry. But it has been made quite plain—so plain that I do not propose to weary you with any reiteration of the well known argument —that ordinary defects of sight, corrigible by spectacles, are mechanical and non-pathological; and that the fitting of spectacles to remove such ordinary defects of eyesight is no more a surgical or medical proceed-

ing than is the adaptation of a pair of boots to fit the feet. More skill is demanded of the Optician, much more intelligence and far larger range of technical knowledge than is required in the case of the boot maker which I have ventured to cite; but I repeat that, so far as regards any claim by the medical profession to regulate and control the retail spectacle trade, there is no more warrant for it than if such claim were advanced with respect to the "artist in shoes." The British Optical Association, I say then, fulfils a most useful function, auxiliary to those of our own Society and of the Spectacle Makers Company; and from what I am told by some who know of the B.O.A. system of examination, it is quite an ideal one for eliciting the technical knowledge and skill possessed by a Refractionist. However, one is compelled to recognise the practical difficulty that there is in the way of procuring men of the highest scientific attainments, whose names carry weight with the public, to lend themselves for furthering the interests of a trading class in our community, and in a direction somewhat resented by many members of the medical profession. I suppose we must, therefore, resign ourselves to waiting, with such patience as we can, for the education of public and professional opinion in this matter. It is a process that is already in operation, although progress is but slow; and it may be, perhaps, not until after some years that we shall find leading Oculists and scientists making themselves responsible for an examination which is avowedly conducted as a test of the proficiency of sight testing Opticians. Such a test is demanded in the interests of the public generally; and I hope that the example and aid of the British Optical Association may have the effect of bringing about such a development as I have indicated, and almost all of us would like to see realised. It is devoutly to be hoped that, undisturbed by individual differences of a personal nature, the several organised bodies representing sections of our industry may come together more and more, in a genuine attempt to further the interests of sight testing Opticians, while at the same time not neglecting such means as scientific lectures, exhibitions, meetings for discussion and so on, calculated to raise the standard of technical and commercial knowledge in the instrument-making trades.

Speaking of the instrument-making trades, the term Optician, which we apply to one employed in connection therewith, is a term that is not infrequently interpreted in too narrow and literal a sense. The Optician surely is any person who is engaged in the manufacture or sale of philosophical instruments in general—not merely eyeglasses

and spectacles, as some purists would have us believe. There are, indeed, very few of the instruments devoted to purposes of scientific research, which do not depend very directly on some application of the laws of physical optics; and even where there is no such dependence, as in the case of many electrical appliances handled by Opticians, the same sort of acquaintance with practical physics, that is so useful to one dealing in telescopes, microscopes, opera glasses, spectacles, cameras, and so on, finds its application. The Photographer's outfit is essentially of an optical character since the lenses and other appliances we use in "painting by light" involve applications of all the principles that we employ in constructing the more severely optical instruments, such as opera glasses and so forth, and even necessitate the further elaboration of these principles to a very considerable extent. There are comparatively few Opticians who do not handle photographic apparatus now-a-days; but this is only another way of putting the fact that to-day everybody is a Photographer. The camera is omni-present; and those who by their scientific training are qualified to handle such instruments intelligently, and to advise their customers on the special applications and use of the various kinds of objectives, yet do not do so, sacrifice deliberately one of the most interesting and lucrative parts of the Optician's trade. I think it is becoming ever more widely recognised that all kinds of instruments of measurement and observation—whether they relate primarily to optics as the field glass, or to electricity as the inductorium, or magnetism as the compass, or heat as the thermometer, to acoustics as the phonograph, to chemistry as the balance, to physiology as the sphygmometer, or to electrolysis as the galvanic cell—all these things are such as it behoves Opticians to concern themselves with. For what retail (or for that matter what wholesale) merchant other than an Optician possesses the necessary technical knowledge to fit him for adjusting or operating and explaining the various instruments in question?

So wide is the scope of all that information which distinguishes hose we term qualified Opticians, that it is hardly to be supposed this could all be attained in any reasonably short course of instruction, nor be attested by any examination for which we can fairly expect members of the trade in general to enter. We must not hope to realise the ideal as yet; we must be content with the accomplishment of those things which are feasible. And if we look to the ground covered by the Spectacle Makers' examination, for instance, while we

regret, as I have said before, that there is no definite reference to sight testing, we should not, I think, regret the fact that only the rudiments of those sciences which it does refer to are covered by the questions set. A general knowledge of applied physics is the very best foundation on which a retail Optician can build up that sound judgment (in such matters as the adapting of spectacles) which is so important for success in his career. We must not suppose that a man's judgment and skill in the affairs of his business or professional life are to be gauged absolutely by any examination. The examination may show and ought to show that a sufficient ground work has been laid, and that the general principles which must guide one in dealing with the problems of refraction have been grasped; but it can scarcely demonstrate the extent to which a student is fitted, by the type and development of his own mind, to apply those principles successfully and with facility when dealing with the complex forms of ametropia met with in every day practice. Even young medical men, at the conclusion of their arduous course of studies, find themselves equipped with a theoretical rather than a practical knowledge of many such disorders as they are, later, called upon to treat. It is learning in the first place, but also experience, that makes the skilled physician; and I venture to say that the same statement may be made with reference to Refractionists also. Let us seek by all means to have the principles of sight-testing explicitly recognised in our examinations; but let us not be misled into supposing that to pass an examination is the chief end and aim of those studies in which young Opticians are counselled to engage.

If it is to these matters, and to the solution of the problems they involve, that we of the Optical Society must apply ourselves, how much assistance may we not derive from an association with those kindred societies professing aims similar to, or identical with our own, and of which I spoke a little time ago? How important it is that we should seek to become even more fully representative of the whole trade than is the case at present, by affiliating ourselves with some of the smaller societies whose zealous and creditable work is so often brought under our attention. I believe, from what I am told by my friends in the provinces, that these societies are quite anxious to become connected with us; and I imagine that the sole reason why they have not yet approached us with a pressing request for affiliation, and a plea for joint action in many matters, has been the existence of some slight misconception which it is in our power to remove. There

is a good deal in a name sometimes, as we all know; and I am told that the term "local section" carries with it, in the opinion of some of our friends, an implication which members of trade organizations distinct from our own would be apt to resent. If such is the case —if my information is correct—surely there can be no reason why we should not substitute for the proposal to establish local sections one of those others which convey less the idea of absorption or loss of independence on the part of those with whom it is our wish to co-operate. The possibilities of vexatious interference by one society with another should be minimised; and I think we all of us admit that a generous scheme of affiliation and the open hand of friendship should be extended by us to the various smaller societies in our own trade. I am sure that this has always been the view held in the Council of the Optical Society; and if anyone is of the contrary opinion, I am convinced it is due to a misapprehension which is, perhaps, largely the result of a mere question of the choice of terms.

It is altogether to publicity in the discussion of these matters that we must look for the elucidation of that modus vivendi under which we can "work out the salvation" of the Trade. And I am glad to say that, just at the present time, there is evidenced on the part of some of our representative men a gratifying readiness to come forward and express their views in the press, and state arguments concerning questions of trade policy. I could wish—and I hope that I shall not be going too far in saying this much by way of criticism—that such expressions of opinion or statements of fact should always be made with due regard for the solidarity of trade interests, by which I mean the close ties of a commercial nature which bind together the retail optical trade, in relation to matters respecting which the public upon the one hand, and the medical faculty on the other, have their own axes to grind. We should try by all means to discover points of unity rather than grounds of difference between ourselves; for if we fall into the natural mistake of carrying too far our expressions of dissent from this or that method used by our neighbours or competitors, we shall put weapons into the hands of those people who profess to think that spectacles and eyeglasses should be sold at the lowest of store prices by every stationer, ironmonger, et hoc genus omne—refusing to recognise the claim of the Optician to be paid on a just scale for skilled professional work. And again we shall strengthen the position of those medical gentlemen whose desire is to substitute, for such chaos in the retail optical trade as is implied by the state of

things I just now mentioned, a system under which the public would seek prescriptions for spectacles only from their medical advisers, while Opticians would be reduced to occupy the menial position of dispensers only. Let us be careful not to "foul our own nest" with danger of such consequences as these. For it is exceedingly clear—at any rate it is so to my mind—that with a strong pull, and a pull together, retail Opticians are now in the position to assert themselves and vindicate the rights of their calling with such effect as has never hitherto been possible. I have said that the trade has developed largely within the past 10 years, which is a fact upon which we may all congratulate ourselves; but I would add that, in my humble opinion, the expansion of trade in the next 10 years should far exceed anything hitherto attained. We have just learned that, for the future, military men are to be freely permitted to wear glasses for the correction of those visual defects from which, as we all know perfectly well, they are by no means exempt. It is a tardy response on the part of the War Office to a call which I myself gave expression to, a number of years ago, and which has been repeatedly urged on the attention of those in authority—notably by our energetic friend, Mr. Laurance. And now that it *is* come, the result is bound to be a radical break down in the absurd prejudice against the wearing of glasses, with which we have had to contend for so long. The prejudice I speak of arises perhaps mainly from the fact that spectacles were at one time regarded as the escutcheon of old age. The idea has, however, lost ground very considerably, since it has become public knowledge that there are many defects of sight in which the use of spectacles is advisable and highly advantageous, even from the earliest years. But there is another deeply-rooted prejudice against spectacles, which finds expression in the idea so long and so obstinately entertained by our military officers, that wearing glasses is in some way inconsistent with a man's efficiency, in that occupation which calls for the fullest expression of all manly characteristics. And I believe that the effect of the new army order, by dispelling this misconception and demonstrating that wearers of glasses are as eligible as other people for all field sports (because even for the great field sport of war itself), will be to break down altogether the last vestiges of dislike to using glasses on the part of the young and athletic section of our population.

Much more might be said, in my opinion, to emphasise the views which I have ventured to bring before you; but I will conclude with this remark, which is, I confess, a favourite remark of mine, that

publicity—publicity which reaches all our fellow citizens—in respect to almost everything concerning spectacles, their adaptation and their use, is that upon which we of the optical trade must rely to bring a speedy realisation of that heightened prosperity which I venture to predict.

Mr. Dixey: Mr. President, we are a very young Society, but we are not so young that we have not our traditions, and one of our best is, I think, that the President's address should be accepted, like some other benefits, I won't say without comment, but without controversy. (Hear, hear.) The President, in the very able address which he has just given to us, has touched on many subjects, upon most of which every individual in this room has his own opinion, and certainly those opinions would not all exactly tally with the opinions which the President has given such able expression to. The remark applies certainly to me as much as to any other individual in the room; but in rising now, it is not my intention—as he has so ably put it—it is not my intention to emphasise differences of opinion, but rather to express my gratitude, and if I may so far presume, the gratitude of other members of the Society too—(cheers)—for having given so able expression to many opinions which we all hold in common. I alluded to the traditions of this Society; and as I say, one of the most pleasant traditions is that the past President should have the privilege on this occasion of proposing a hearty vote of thanks to the President for his inaugural address. (Cheers.) This vote I have the pleasure of proposing now. If I make any further observations on one or two subjects which our President has alluded to, I will ask you to remember that it is not by way of controversy at all, but simply if I can, to strengthen the suggestions that the President has made. I was very glad indeed that he mentioned—in talking of our social meetings—that he advocated and put forward the view that interest in them would be very much enhanced if we had more exhibits of instruments, and so on. (Hear, hear.) It is a strong point, on which I am sure we all agree. Those members of the Society who are in a position to make exhibits of this kind want some such little variety as our President has mentioned, and I think it is the duty of all of us, who have it in our power, to increase the attractiveness of our social gatherings in that way. It is the duty of all of us to take that observation to heart, and to carry it out to the best of our means. The President has contrasted the condition of the optical trade now with its condition 10 years ago. I fully agree with the opinion that he expresses. That is really my experience of the optical trade, which goes back

now for 25 years, and I believe I am quite right in thinking it is really healthier and more active than it was 10 years ago—(Hear, hear)—and that is not a thing one can say of many trades. Lately there have been certain things not altogether satisfactory; but, taking it generally, all round, we are now in a better position, with more promise for the future than one could have said 10 years ago. (Hear, hear.) The President, in speaking of the defects of sight, has divided the defects into two classes, pathological and mechanical. I hope you will not consider it controversial if I say that I do not altogether accept the words. I do not think they include everything, and I do not altogether like the expression. It is very difficult for me to sit down and hear it put exactly like that. I want to suggest to him with reference to the word "pathological"—what he means by that—that he should choose a better word, and then I should agree with him cordially. A word such as "morbid." As a matter of fact mechanical defects of sight are, with possibly very few exceptions, pathological. Almost every defect of sight is pathological in the general acceptation, and the literal meaning of the word, but there are certain conditions of sight which you may regard as defects of sight, which are morbid; and, therefore, we shall all, I think, agree that they certainly ought never to be touched by an Optician. With regard to the co-operation of the other societies, I think the question is one a little bit difficult for us to approach. We have a large number, and we value our country members, the members that come to us from other towns and the provinces—(Hear, hear)—but after all we are mainly a London Optical Society; the advantages that our Society has to offer are principally available to the London members; and after all I can fully sympathise with any man living in such a town as Hull, Bradford, or Manchester (I must not name the towns lest I leave some important one out, but the large northern towns), I can fully enter into the feelings of a citizen of one of those towns saying to me, "What good do I get from this Optical Society; when I have an Optical Society in this important centre?" York, say, or Liverpool or Manchester. That does not exclude co-operation. But after all, I want you to think of the thing definitely. How are we to co-operate? What can we do except to have cordial feelings for each other? We can agree with the Optical Society of the city of York or Manchester, but when it comes to definite ideas of co-operation what can we do? Are we going to allocate a certain portion of our funds to help the Society in Manchester, or is the Society in Manchester going to help the Society

in London? I do not think so, gentlemen. But at the same time that does not alter our feelings for any reciprocity that can be displayed. Referring to the military use of glasses, it is quite right that we should be thankful for it. It is a small mercy, but we ought to be grateful. But one important thing has not been done yet, and that is a modification of the optical test for admission to the army. That is a thing that really, in my poor opinion (it must go for what it is worth), is a most important thing that wants reconsideration and reorganisation. Possibly, that is a blessing to come. I must apologise, Sir, for having spoken at rather greater length than perhaps I should have done, and in sitting down I remind you of my object in rising, which was to propose a hearty vote of thanks to the President for his very able address. (Cheers.)

Dr. Walmsley: I should like, Sir, to be allowed to second that vote of thanks. I do not know whether the tradition to which Mr. Dixey has alluded extends to the past President once removed, but I should think it would be a nice tradition to establish if it could be, that the duty of seconding the vote of thanks to the President should fall upon that particular officer each year. I may have a selfish motive in suggesting that, as if it were the custom I should not be called upon to officiate in this way again. However, I should not say that, Sir, because it is a distinct pleasure to me to be allowed to second this vote of thanks. Mr. Dixey has told us that the subject of the President's address is not a subject for discussion, and that one of our traditions is that it should not be discussed in the same way as a paper; and that tradition is common to every society with which I am acquainted. Nothwithstanding that, Mr. Dixey has allowed himself to make one or two remarks upon the address, and if you will allow me, I also will make one or two. I shall try to refer to even less controversial subjects than Mr. Dixey did. That subject of Local Sections, for instance. I am rather surprised to learn that the title is a stumbling block; but I accept the statement that it is so. But it has not proved a stumbling block in other societies. The word was not coined by the Optical Society—it is in wide use already. And if I may, I should like to suggest one method of co-operation, provided the nomenclature can be altered, and I have no objection to altering it in any way you like. One method of co-operation is that the papers which are read before the provincial or other societies which may hereafter be affiliated to the Optical Society—that those papers be published in our proceedings, and thus be brought before a wider circle of readers than

if they simply appeared in the proceedings of a single society. In that way I would suggest there is a distinct line for co-operation between the Optical Society and its brother societies in the provinces. I should also like to emphasise the remark made not only by our President, but by Mr. Dixey—that we should like to see at our meetings exhibits of new instruments with which some of us in our work have not the opportunity of becoming acquainted. And I would tell you that a notice to this effect appears regularly upon the official notices that are issued by cognate societies. For instance, in the case of the Physical Society of London; almost every notice that goes out, at any rate with reference to meetings, has a footnote that the Society is always willing and glad to receive at its regular meetings exhibits of apparatus which its members may bring forward, even though this apparatus is neither original nor new. And I would like the members present to take notice of that proviso—that it is not necessarily something new that we want to see; not necessarily something original in the man who brings it forward, but we want to extend the usefulness of the Society—to have exhibits from time to time of apparatus with which, as I said before, we are not, all of us, familiar or get the opportunity to become so, however ancient it may be. (Hear, hear.) And now, Sir, I should like, in concluding the few words I have to say, to express my personal thanks for the very able review you have given us of the history of the trade. Whatever opinions we may hold about the various controversial matters, we all of us must, I think, be thankful to have put into concise form the history of the optical trade for the last 10 years. (Hear, hear.) I, Sir, feel that I am fallen under an obligation, for I have not—I confess it; I do not say that I blush for it—but I have not been acquainted with the history of the optical trade for that length of time. My duties have led me to Scotland and India, and I have not had the chance of being in London for long during the last 10 years, and I am very glad indeed to have available for future reference this history; because I presume the vote of thanks will be attended with the request that the President will allow his address to be printed, and, in due course, circulated amongst the members of the Society. (Hear, hear.) I, therefore, gentlemen, very cordially second this vote of thanks, and as our President cannot put it to the meeting, I put it to you. Is it agreed? ("All.") It is unanimously agreed, Mr. President, that we accord you a hearty vote of thanks for your address. (Cheers.)

The PRESIDENT: Mr. Dixey, Dr. Walmsley, and gentlemen, I thank you very sincerely indeed for the extremely cordial vote of thanks you

have passed on the matter of my address. I was exceedingly doubtful as to the advisability of introducing into my address matter which Mr. Dixey has criticised as controversial. Well, it is of course more or less controversial, because everything in this world is controversial —(Hear, hear)—but as Mr. Dixey has said, every man in this room,— I have his words here—every individual in this room has his own opinion. I do not know whether he meant that we were likely to be divided into two parties on the questions I have raised; but my opinion with regard to many of the matters touched upon is that every individual—and I gather that there are something under 200 of our members here to-night—(laughter)—but if there were the whole of our hundreds of members present here, they would carry into effect what Mr. Dixey has told you, and each have a different opinion on almost every subject I have dealt with. Therefore, gentlemen, I do not apologise on the score that my remarks were controversial. But notwithstanding his intimation that it was improper to criticise the address of the President, Mr. Dixey touched on one subject upon which I would wish to put myself right. It was the question of defects of vision, as to these being divided into pathological and mechanical defects. Now what I really wished to imply, whatever my words were, was that the treatment of defects of the eye might be divided into pathological and mechanical treatment. Mr. Dixey told you that "morbid" would be a better description of the diseased conditions of the eye than "pathological," but if "morbid" is not included in "pathological" then I am rather confused over these same terms. My point was rather this, when I spoke of mechanical treatment of the defects of the eye—that is to say, treatment of mechanical defects —I made reference more particularly to such physical defects as might be classed with muscular deficiency of any part of the body, or bowed legs, and kindred things of that description, which do not come under the purview of a medical man at all. If a child has bowed legs, the recognised treatment is to take him to an instrument maker. If a man is dissatisfied with his muscles, he goes to Sandow or some kindred school of muscle training. (Laughter.) However, I am not going to touch further upon this subject, and it only shows how the mere slip of a word may place a totally different construction upon a word or a phrase than was primarily intended. I want to thank you again for the exceedingly kind manner in which you have received me to-night, and for the compliments with which you received the vote of thanks accorded to me. (Cheers.)

THE TESTING OF OPTICAL INSTRUMENTS.

PAPER BY DR. CHARLES V. DRYSDALE, *Member.*

Read before the Optical Society, May 15th, 1902.

IN bringing before you a paper on the Testing of Optical Instruments, it is evident that it will be impossible for me to do more in the time at my disposal than touch generally upon so large and important a subject, and to mention a few particular details which may appear of special interest. A long treatise might be occupied if the subject were adequately dealt with, but I prefer to deal with it generally rather than with a specific portion, because it is one above all in which general knowledge is necessary among Opticians, and is somewhat difficult to obtain owing to the lack of any connected treatise on the subject. The main object of the paper is to bring to the notice of Opticians concerned in the supply of instruments such as field glasses and telescopes, microscopes, projection lanterns, photographic apparatus, etc., a general idea of the defects to which such instruments are liable and how they may most readily be dealt with. For this reason, I shall not lay so much stress upon very accurate scientific methods of testing as upon those which are readily performed. It must be understood, also, that I only propose in this paper to deal with the testing of defects in the optical portions of instruments and not with those in mechanical construction.

General Principles.

The function of all optical instruments, however diverse they may be in other respects, is finally to produce an image, either real or virtual, of some object, and to produce this image with the greatest possible accuracy of detail in every particular, usually with a certain amount of enlargement or reduction of dimensions. For this purpose, use is made either of plane or curved mirrors, refracting or reflecting prisms, and lenses; and any optical instrument is simply an assem-

blage of one or more of these items. It has been shown that any perfect optical system acts, so far as the production of an image is concerned, as if it were a single lens, having a definite focal length and two definite points on its axis known as the principal points, through which pass the principal planes or planes of unit magnification. Fig. 1 illustrates such a system showing the two principal points and planes, P_1 P_2; the two focal planes, F_1 F_2; and also the two symmetrical planes, S_1 S_2, the latter being such that an object in one of them produces an inverted image of the same size in the other. Any one of the distances, $S_1 F_1$, $F_1 P_1$, $P_2 F_2$, or $F_2 S_2$, represents the equivalent focal length. It is therefore of value, generally, to know the value of the focal length of the system and the positions of these principal points (which are usually coincident with the nodal points). Beyond this, however, we frequently require to know the magnification produced by the instrument, or the ratio of the size of the image to that of the object. Furthermore, we are concerned in knowing the angle of view which the instrument will embrace, or the angle between the extreme rays of light which can or do enter it. Not only, also, is the production of the image in itself of importance, but its brilliancy or illumination concerns us considerably, and in order to know this we must determine what is called the aperture of the instrument. Finally, for some purposes it is of value to know the curvatures of the various surfaces.

It should be remembered, however, that no optical instrument produces a perfect image, and the images produced suffer in clearness from various defects, some of which are due to defective quality of materials employed, others to defective workmanship of the optical surfaces and to faults in their mounting, while a third class of defects are inherent in the optical properties of the surfaces of the lenses and mirrors, and depend upon the proper designing of the system. We may, therefore, divide the defects to be examined into three headings:—

(1) *Due to defects in material.*—Since glass is most extensively employed in optical work, the defects in its constitution are the most serious, and they may be enumerated as existence of veins or striæ and bubbles in the glass itself; want of transparency, either initially or from subsequent chemical action; or strain, consequent on imperfect annealing of the glass during manufacture.

(2) *Defects in workmanship.*—In this class we first have imperfect polishing, causing scattering and loss of light; errors of form, usually

in departure from the plane or spherical form: errors in the parallelism of plane glasses or of the angles or prisms; and, finally, defects in the centering of the different lenses or mirrors in the system.

(3) *Defects due to design.*—Under this heading, we have the usual errors which are well known, viz.:—

(*a*) Chromatic aberration, in which different colours are brought to different foci. Fig. 2.

(*b*) Spherical aberration, in which the rays of light parallel to but at a distance from the axis are brought to a different focus than those near to the axis. Fig. 3.

(*c*) Coma or spherical aberration for oblique pencils, producing a one-sided flare on the image. Fig. 4.

(*d*) Radial astigmatism, in which oblique light is focused not at a point but in two focal lines mutually perpendicular. Fig. 5.

(*e*) Distortion, in which straight lines in the object appear curved in the image. Figs. 6 and 7; and

(*f*) Curvature of the field, in which the best image of a plane object perpendicular to the axis is formed on a curved instead of a plane surface. Fig. 8.

Among the defects due to design may also be mentioned inequality of illumination, generally causing a reduction of brilliancy of the image as we pass outwards from the axis; and that known as flare spot, causing light reflected from the surfaces of the lenses to be approximately focused on the image.

It will be seen from this brief survey of the points involved in optical testing that the subject is an exceedingly large and complex one, especially when we are further concerned in the adaptation of the tests to the very varied types of instruments in use. Nothwithstanding this, however, a general discussion on the tests necessary will not be beyond our scope, and will obviate the necessity of doing more than give details in dealing with the specific requirements of different instruments.

Section I.—General.

A. *Determination of the Equivalent Focal Length.*—In the case of mirrors, single thin lenses, or of combinations of lenses, the total thickness of which is very small in comparison with their focal length, the latter may usually be taken with sufficient accuracy as the distance from the surface of the mirror or lens to the principal focus. Where

the combination is convergent the focal length is immediately determinable; if divergent, the focus is virtual and must be found either by neutralisation or by a telescope or range finder, or by combination with a convex lens of known shorter focal length. Little difficulty, however, need ever be experienced in such cases.

Where, however, optical systems are of considerable axial length compared with their focal length, as in most photographic lenses, eyepieces and microscope objectives, the distance between either extreme surface and its corresponding focal point is not the equivalent focal length, but what is technically known as the "back focus," and may be very widely different from the former. In photographic lenses, for instance, the back focus usually roughly corresponds to the camera extension, while the equivalent focal length is greater, indeed in telephoto lenses it may be many times as large.

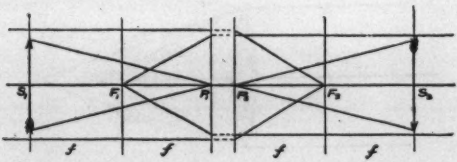

FIG. 1.—PRINCIPAL, FOCAL AND SYMMETRICAL PLANES.

It would seem unnecessary to give special attention to the testing of the equivalent focal length, but unfortunately even now mistakes seem frequently to be made on this important point, and even some of our recognised text books contain serious errors. An impression which seems to die hard is that the equivalent focal length of a photographic lens is the distance between the diaphragm stop and the screen, but this is only occasionally the case. Another is that if the lens is so set as to give an image equal in size to the object, the equivalent focal length is one-fourth of the distance between them. This is only true if the nodal points are coincident.

There are many well-known methods of truly determining the equivalent focal length, but they divide themselves into three groups. In the first of these the distance between the principal or nodal point and the corresponding focal point is determined to which class belong

the equivalent lens test, and the tests of Sir H. Grubb,* Moessard,† the Kew Observatory,§ and the mirror test of Dr. Clay.‡ In the second class the distance between the focal and symmetrical planes is directly determined ; while in the third class we have the modified method of conjugate foci, and the magnification tests of Dr. S. P. Thompson and Professor T. H. Blakesley.

Of these only two will be described as being probably the most suitable in practice. The first, which I shall term Moessard's method, from its having been brought to perfection by him in his Tourniquet, is very old in principle, and consists of simply rotating the optical system on an axis perpendicular to its own axis, and to find the position by trial when on slightly oscillating the system on the axis the image of a distant object remains stationary. This can only be the case when the axis of rotation cuts that of the system at the nodal

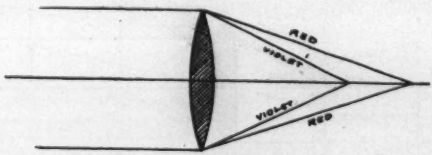

FIG. 2.—CHROMATIC ABERRATION.

point of emergence, and the distance between the axis and the focus is then the equivalent focal length required. Fig. 9 shows a simple method of carrying out the test, in which a lens system to be tested is laid in a trough supported on an oscillating axis. When a clear image of a distant object is formed on the screen, and the trough is given a slight oscillating motion, the image will generally be seen to move. If, however, the lens is moved backwards or forwards over the axis, and the screen is correspondingly moved, a position will readily be found in which a sharp image is produced which does not move for

*R. S. Cole, "Treatise on Photographic Optics," p. 218. Trail Taylor, "Optics of Photography," p. 116.

†P. Moessard, "Etude des Lentilles et Objectifs Photographiques," Paris, 1899. Cole, "Photo Optics," p. 209.

§Leonard Darwin, "On the Method of Examination of Photographic Lenses at the Kew Observatory," Proc. R.S., 1892. Cole, "Photo Optics," p. 224.

‡Dr. R. S. Clay, "Testing of Photographic Lenses," 'Photography,' February 14th, 1901.

small oscillations. The focal length required may then simply be scaled off from the axis to the screen. The test in this form is, of course, only applicable to convergent combinations having a positive focus, but in the case of negative combinations, such as ordinary microscope or telescope eyepieces, or divergent combinations, the writer has modified it in connection with his universal optical bench described before the Society.*

The other method is that by which the focal length is obtained from the distance between the focal and symmetrical planes. In this test as applied to positive convergent systems, a distant object is first focused sharply upon a screen. An object is then brought near the lens and is moved until its image is equal in size to itself and is perfectly sharp. The distance which the screen has had to be moved from the first position to the second is the focal length required.

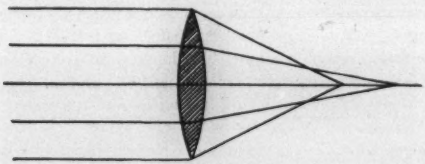

FIG. 3.—SPHERICAL ABERRATION.

Further reference to this subject will be made in the special sections.

B. *Position of Nodal Points.*—It would certainly be a great convenience in practice if the positions of the nodal points of optical systems were marked on their mounts, as has been done for some time by M. Moessard, and recently again advocated by Prof. Blakesley. In Moessard's test the position of these points are directly given, while if the equivalent focal length is determined by any other method, and the position of the focal planes is known, the positions of the nodal points can, of course, be found at once.

C. *Determination of Magnification.*—The term magnification is one which is employed in at least three different senses. Scientifically it is defined as the ratio of the linear dimensions of the image to those of the object, and this definition is used when speaking of projection

*A simple universal method of testing spherical and cylindrical lenses. Proc. Opt. Soc, May, 1900.

lenses or microscope objectives, but in telescopes it implies the visual angle which the image of a small object subtends, compared with that which the object would subtend at the same point. With simple microscopes, or with oculars, however, the term magnification has a third meaning, and implies the visual angle subtended by a small detail of the image compared with that subtended by the same detail of the object, the image and object being in each case situated at the distance of most distinct vision, usually taken at 10 ins. or 250 millimeters. The testing of magnification will, therefore, be deferred until the special instruments are dealt with.

D. *Angle of View.*—The angle of view is again a quantity which is variously defined. It may be the angle between the extreme rays which can pass through the system, as in a telescope or microscope; the angle between the extreme rays with which the system is actually used, and which varies with the conditions, as in photography; or the angle between the two extreme directions over which a certain standard of definition is attained, or over which the illumination does not vary by more than a certain amount. The latter definition corresponds to what is called the covering power or angle of a photographic lens, or to the "angle of aplanatic cone" in a microscope condenser.

E. *Determination of Aperture.*—The term aperture has two different significations in the case of telescope or photographic lenses on the one hand, or of microscope objectives on the other. With the former the aperture implies the ratio of the diameter of the emergent pencil of light to the equivalent focal length, while with the latter it is usually taken as the sine of half the angular aperture multiplied by the refractive index of the medium in front of the objective. Both of these quantities can be readily determined and will be described in place.

F. *Curvature of Surfaces.*—Little need be said on this subject. For most purposes the spherometer, especially in Prof. Abbé's form, is suitable, but when the diameter is too small, or curvature too large, the method of testing devised by the writer* may be substituted.

G. *Tests for Defects in Material.*—These tests are well known and need only be briefly referred to. In order to detect veins or bubbles in the glass, the most perfect method is to arrange for a cone of convergent light to pass through the glass and enter the pupil of the eye so that the whole of the glass appears as a brightly illuminated field.

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* "A Simple Direct Method of Determining the Curvature of Small Lenses," Proc. Opt. Soc., November, 1900.

If the glass is already ground into lens form it can be used to converge the light, as in fig. 10, otherwise an auxiliary perfect lens may be employed; under these circumstances any such defects are made clearly apparent. The existence of strain or imperfect annealing of the glass may be immediately detected by interposing in the path of the light a pair of ordinary polarising and analysing prisms, set so as to give a dark field, any regular strain of the glass immediately shows as a light or dark cross on the field. Irregular strain is shown by the figure being distorted.

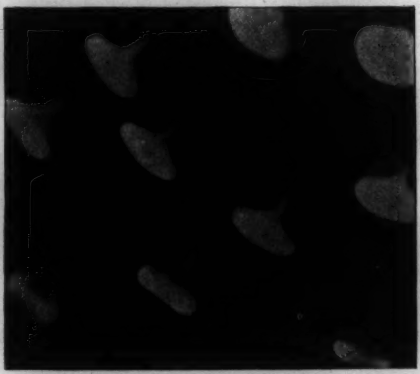

FIG. 4.—COMA.

Defective polishing of the surfaces is, of course, easily detected by examining the surface in a strong reflected light with an ordinary magnifying glass.

The only simple way of testing the transparency of the glass is to compare the piece to be tested with a satisfactory piece of glass, by holding them side by side against a uniformly illuminated sheet of white paper. For accurate testing a photometer should be employed, but apart from the inconvenience of this it should be remembered that the indications of a photometer afford no clue to the transparency for actinic light.

Tests for Errors of Form.

Plane surfaces may be accurately tested in either of two ways, first, by using a telescope which has been focused on some distant object, and afterwards observing the same object by reflection at the surface to be tested. A very slight imperfection of the surface will destroy the sharpness of the image. A more convenient method in the workshop is to have a standard plane surface, either of glass or quartz, and to observe the coloured bands produced on pressing the test surface into contact with it, having previously taken care to thoroughly clean the surfaces. If the surface tested is accurately plane the bands will be very wide and regular, appearing either as straight lines or circles. With the highest possible perfection only one colour will be seen over the whole surface.

The parallelism of plane glasses is frequently a point of considerable importance, particularly where these glasses are to be used in front of

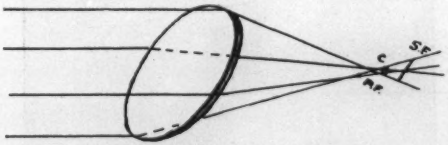

FIG. 5.—RADIAL ASTIGMATISM.

some measuring instrument, as the slightest error from parallelism causes a deviation of the light entering the observing telescope. The most simple test for parallelism is to view a source of light by oblique reflection at the plate tested, when, if the plate is perfectly parallel, the image only will be seen. If, however, the surfaces are not parallel a series of images will be seen, and the greater the angle between the surfaces the further apart will these images be.

A second quick method is to sight an ordinary telescope on a distant object, such as a vertical flagstaff, and to hold the glass to be tested in front of the object glass so that it half covers it; in this case if the glass is parallel the object will appear as before, but should there be want of parallelism the image will appear double in some positions of the glass.

With deeply tinted glasses, such as are used in sextants and other nautical or surveying instruments, some difficulty may be experienced.

A simple expedient in this case is to employ a fixed telescope with cross wires and to accurately focus some object on the wires. If the glass is now rotated in front of the telescope any want of parallelism will be shown by the image also rotating.

The angles of prisms may of course be most accurately tested with the help of the spectrometer or goniometer, but in some special cases it is convenient to test them by a simple internal reflection from the prisms themselves; for example, in prismatic field glasses, and many other cases, right angled prisms are employed, and such prisms may conveniently be tested by looking at the broad face of the prism, in this case an image of the observer's eye will be seen by successive reflection of the two faces, one half of the pupil will be seen on each of the oblique faces. If the angle of the prism is exactly a right angle the pupil will appear to be perfectly circular, but otherwise the two halves will appear to be separated or to overlap, according to whether the angle is greater or less than a right angle. Tests of a similar nature may be made with prisms of other definite angles.

Curved Surfaces.

In lenses and mirrors the surfaces are usually in the form of spherical curves, and it is of the greatest importance that they should be perfectly regular and free from astigmatism. In the workshop the curves of the surfaces are usually checked during construction by the use of test plates ground and polished to the corresponding curves. If the coloured bands formed under these circumstances are perfectly circular and are unaffected by the movement of the surfaces over one another, the glass may be considered to be truly spherical. Where figuring is intentionally resorted to an optical method of test must be employed.

Centering.

The centering of any system of lenses or mirrors is a matter of very considerable importance, as an otherwise perfect instrument will have its definition spoiled by faulty centering. When the instrument is completed the most usual quick test for centering is by Wollaston's method, in which a distant point of light is observed through the system in a darkened room. In this case a number of images will be seen by reflection at the various surfaces, and if by tilting the system all of these images can be brought into one line, the system may be regarded as properly centered, otherwise the tilting or displacement of

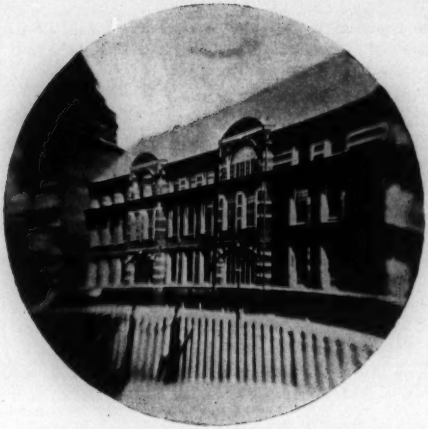

FIG. 6.—PINCUSHION OR POSITIVE DISTORTION.

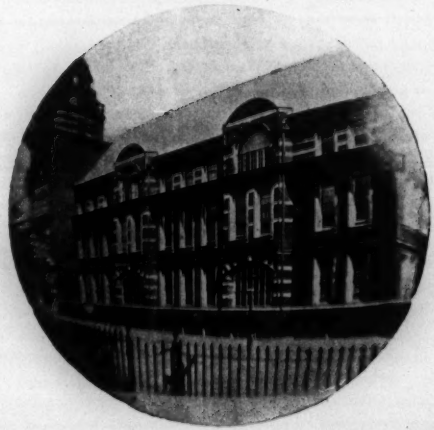

FIG. 7.—BARREL OR NEGATIVE DISTORTION.

any surface causes a corresponding displacement of the image reflected from it. In practice it is usually somewhat difficult to carry out this test, as the image directly seen through the system is often so bright as to mask the other images, and I have found it convenient in performing it not to look directly through the lens system, but to reflect the light into it from a piece of plain unsilvered glass held in front of the eye. Under these circumstances the whole of the images are reflected, and are consequently nearly equally intense, and the test is much easier.

In order to test the centering of the whole of an optical system in its mount, it is sometimes convenient to rest the latter on a couple of V bearings so that when rotated in these bearings the mechanical axis of the mount is stationary. Under these circumstances if an object is viewed through the lens system, with or without an eyepiece, or an image is projected on to a screen by its aid, and the tube is rotated, the image should be perfectly stationary if the centering is perfect, otherwise it will appear to describe a smaller or larger circular path.

Lenses are usually centered in the workshop and edged truly by mounting them on a lathe and observing the reflections from the two surfaces, the lens is moved on the axis until these images appear perfectly stationary as the lens revolves, and the edging is then proceeded with. Lathes for edging lenses have also commonly hollow mandrels, so that the test previously described may also be employed.

In centering lenses which have to be cemented together, the same procedure as in the last case may be adopted, but some optical manufacturers, notably Messrs. Goerz, employ a mechanical method of effecting this, consisting of a lever with a small spherical head at one end touching on the surface of the lens, and a long pointer which shows any variation of the level of this sphere. The lens to be tested is laid on a flat table against two supports so that it may be rotated by hand about a vertical axis, and the lens is adjusted in contact until on rotation no movement of the lever takes place.

The final testing of centering will be described later.

Defects due to Design.

(*a*) *Chromatic Aberration.*—The tests for chromatic aberration are fairly simple and well known, and little need be said about them. A single lens, of whatever material it is made, always brings the violet

light to a focus at a shorter distance than the red light, intermediate colours being focused between the two extreme positions. The use of a compound lens formed of glasses of unequal dispersive powers compensates for this to a considerable extent. If this correction is insufficiently performed the lens acts like a single one, but in a less degree, and the combination is then said to be under corrected, while if the correcting lens has too great an effect, causing the violet light to be focused at a greater distance from the lens than the red light, the combination is said to be over corrected. It is, therefore, possible to ascertain whether a lens is over or under corrected by finding the positions of the foci for violet and red light, but the correction of the lens may be much more easily tested by observing the disc of light

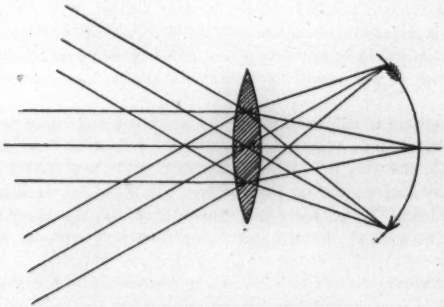

FIG. 8.—CURVATURE OF FIELD.

produced on a screen, or in an achromatic eyepiece, when a distant source of light is observed and the screen is inside or outside the focus. If the screen is inside the focus and the lens under corrected, the violet light is more refracted than the red, and consequently the extreme edge of the disc of light is coloured with red, while if the lens is over corrected a blue rim will be seen, the reverse effects will, of course, be observed outside the focus. Other tests for chromatic aberration will be mentioned when dealing with the special instruments. It should here be mentioned that chromatic correction of axial light by no means necessarily implies entire correction for oblique light.

(*b*) *Spherical Aberration.*—The defect of spherical aberration is a very common one in optical systems, and implies that the edge of the lens or lens system has a higher or lower refracting power than its centre. With a single lens the focal length for light passing through the edge of the lens is always less than that for the central light, and with a compound lens it may be either less or greater; in the former case the lens would be under corrected for spherical aberration, in the latter over corrected.

The simplest rough test for spherical aberration is to focus the lens on a screen so as to give the sharpest image possible, using first a stop which covers up all but the middle third of the lens under test, and secondly with a stop in the form of a disc which covers up the centre and leaves the edge free. If the focus remains unchanged with the two stops, spherical aberration is not present to a very large extent, but if any movement of the eyepiece has to be made to restore

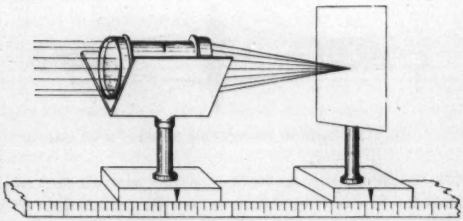

FIG 9.—TESTING EQUIVALENT FOCAL LENGTH.

focus when changing stops, it can be easily seen whether the lens is under or over corrected. This test is by no means sufficiently accurate for telescopes or photographic lenses, and more sensitive tests will be described later. The remaining errors of coma, radial astigmatism, distortion, and curvature of the field, are so particularly connected with photographic lenses, that tests for them will be deferred until this section is reached.

We may now pass to the application of the tests to commercial Optical Instruments, and I have selected four classes of instruments for special attention, *i.e.*, Telescopes and Field Glasses, Photographic Lenses, Projection Apparatus, and Microscopic Lenses.

Section II.—Telescopes, Opera and Field Glasses.

In dealing with these instruments, we are very nearly always concerned only with a very narrow angle of view, and consequently the various errors arising from oblique refraction are of comparatively little importance, and, provided the centering is perfect, the points which we are most concerned in testing are, therefore :—

(*a*) The centering, or "squaring on,"
(*b*) The magnification of the glass,
(*c*) Its angle or field of view,
(*d*) The illumination of the image,
(*e*) The chromatic correction, and
(*f*) The spherical aberration of its object and eye lenses.

(*a*) *Centering.*—The only point we have here to deal with, after what has been said in the general section, is what is technically known as "squaring on." Telescope object glasses are usually very

FIG. 10.—METHOD OF TESTING STRAIN IN GLASS.

sensitive to tilting, any inclination of the lens causing coma or astigmatism. The test for tilting is very simple, and like all other tests of the optical quality of a glass, is best made on a star or other distant source of light. If this is not convenient, a valuable substitute can be obtained with a "collimator," which consists of a telescope having an object glass, at any rate, larger and more perfect than any to be tested, and having at its focus a diaphragm with an exceedingly small pin hole. This arrangement serves as an artificial star, since the light emerging from the pin hole is rendered parallel after passing through the lens.

When a real or artificial star is available, the glass to be tested is simply focused on it, and is then racked in and out of focus. If the disc of light which is seen when out of focus is always perfectly circular, the glass is properly centered, but if an unsymmetrical flare is seen, or the disc is oval, the objective is either tilted or is astigmatic.

(*b*) *Magnification. Double Vision Method.*—The magnification of a telescope may most conveniently be defined as the visual angle which a small object subtends when seen through the telescope, compared with that which it subtends with the unassisted eye. The simplest possible method of testing the magnification is, therefore, to observe a distant scale through the glass with one eye, and to notice how many divisions of the scale seen by the other eye are covered by a single one seen through the glass. If means are provided for holding the glass steadily, this may be fairly satisfactory for low magnifications, but it is unsuitable if great accuracy is required, or if high magnifications are to be measured.

By Measuring Focal Lengths.—It can be easily proved that the magnification is equal to the result obtained by dividing the focal length of the object glass by that of the eyepiece, and in the case of opera glasses of low magnification, this affords a method of testing the magnification. The focal length of both of the lenses can be easily obtained by an optical bench or by neutralisation, and the magnification obtained in this way; the result cannot be expected, however, to give any accuracy with high magnifications, as in that case the eye lens is of short focus, and accurate neutralisation is impossible.

Ramsden's Method.—When parallel light enters the object glass of a telescope or opera glass focused for an emmetropic eye, it is parallel on emergence, and it can be proved that the magnification is equal to the diameter of the entering beam, *i.e.*, the aperture of the object glass divided by the diameter of the emergent beam (sometimes called the diameter of Ramsden's circle), *provided that the light is not intercepted by stops in the interior of the telescope.* This method appears to be very frequently used for determining the magnification of telescopes and field glasses of high magnification, such as prismatic glasses; a distant small source of light is focused in the glass, and the emergent beam of light is allowed to fall on a ground glass or celluloid scale, and the magnification is obtained by dividing the diameter of the object by the diameter of this disc. A further refinement of this test is to measure the diameter of the Ramsden circle by means of an instrument devised by Ramsden himself, known as the Dynameter. In the author's opinion, however, this test is by no means satisfactory, as any stopping down in the body of the glass tends to reduce the size of the emergent pencil, and consequently to give a fictitious high value to the magnification. It is obvious that if a glass, having a certain magnification, were to be provided with a cap on the eyepiece, cutting

down the diameter of the beam to half, the test would apparently give a magnification double the true value.

Author's Method.—None of the methods above described or that seem to have been devised, appear to be altogether satisfactory for universal application, and the author has, therefore, been led to devise one, which, although it requires a little subsidiary apparatus, can be easily set up and is capable of giving directly the magnification of any glass to a fair degree of accuracy. The principle of the method will be easily seen from fig. 11, where A is an ordinary single telescope used as a collimator with a slit which can be illuminated from any convenient bright source of light, and which is adjusted in the focal plane. If a second convex lens of about 50 centimeters focal length is placed in front of the glass with a screen at its focal distance, a sharp image of the slit will be formed on the screen as the parallel light is focused by the lens.

Let us now suppose that we have two slits, or more conveniently that

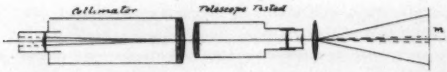

FIG. 11.—DIRECT METHOD OF TESTING TELESCOPE MAGNIFICATION.

a Maddox double prism is interposed between the slit and the object glass, the result will be to give two beams of light which will consequently be focused by the second lens and will give us two images of the slit instead of one. By sliding the prism nearer or further from the slit, the images formed on the screen may be made to approach or recede from one another, and it is quite easy to set the prism so that the distance between the images is some definite amount, say one centimeter. When this is the case, let any glass whose magnification is required be simply focused on a distant object, and then interposed between the object glass and the lens, the effect of the telescope is to increase the deviation between the two beams of light received by it by an amount equal to the magnification, and consequently the two images of the slit will be seen as before, but they will now be as many centimeters apart as the magnification of the glass. All that is necessary, therefore, is to interpose any glass in the space between the object glass and the focusing lens, and the number of

centimeters distance between the images is the magnification directly, without any calculation. By suitably proportioning the apparatus, it is possible to test magnifications of almost any glass in this way, and the convenience of the method is that it is visible to more than one person at a time, and that, therefore, there is no question of personal error.

(c) Determination of the Angle or Field of View.—The angle of view of a telescope or field glass is usually given as the number of degrees or minutes of angle between the two extreme rays which are visible through the glass. The most convenient method of testing this, when a sufficient distance is available, is to set up a scale at such a known distance that the telescope is focused at its ordinary working length, and see how many of the scale divisions are visible in the field. If now the length included between these extreme divisions is divided by the distance between the scale and the object glass of the telescope, it gives the angle of view in radians, and this multiplied by 57·3 gives the angle in degrees, or if multiplied by 3,430 gives the angle in minutes. If much testing is to be done, it is convenient to set the scale up at a distance of 3,430 scale divisions from the object glass; in this case the number of divisions visible in the glass at one time gives the angle directly in minutes.

It is, however, frequently very inconvenient in practice to have to test glasses on a distant object from considerations of space, and here again, a collimating telescope may conveniently be employed. If a properly corrected telescope lens of any convenient focus is employed, as in the test before mentioned, and instead of a single slit in the focal plane a scale is employed, a glass focused for parallel rays will, of course, see the scale clearly, as if it were at an infinite distance, the scale may easily be divided so as to indicate degrees or minutes of angle at the optical centre of the collimator; in the former case, each division being 1/57·3, and in the second case, 1/3430 of the focal length. Under these circumstances, the test is subjective, but if used with a projecting lens and screen as before, the divisions will be clearly focused on the screen, and the number of them indicates the field of view. This test is objective and very convenient, the only point is that a sufficiency of light must be allowed for projection. It should not be forgotten, also, with opera glasses of low magnification, that the field of view depends on the diameter and position of the pupil of the eye.

When the angle of view of a Galilean glass is not restricted by

stops in the interior, it may be found from the formula—angle $= \frac{p f_1 + d f_2}{f_1 (f_1 - f_2)}$ where f_1 and f_2 are the focal lengths of the o.g. and eye lens respectively, p the pupillary diameter, and d the diameter of the o.g., provided that the eye is close to the lens.

(d) Illumination.—The illumination, apart from absorption of the lenses or mirrors, is easily found in the case of a telescope, and may be expressed by dividing the diameter of the Ramsden circle by that of the pupil of the eye and squaring the result. The illumination, of course, can never be greater than unity.

Examination for Defects.—It is only proposed here to mention the simpler tests for defects in glasses of low magnifying power, as a discussion of those in larger glasses would take too much time, and is rendered unnecessary by the fact that an excellent little book on the testing of astronomical telescopes has been issued by Messrs. Cooke.*

The best object for detecting any defects in the performance of a telescope, as above mentioned, is unquestionably a star or distant bright point of light. Among Opticians it is customary to test glasses on test types or print at a considerable distance, and this may give an excellent idea of the performance of a glass, but it is almost useless as a guide to determining to what faults any defect in definition may be due. The test types, or similar device, should be always used as a means of judging the capacity of a glass, but if any more detailed information is required as to the defects, the luminous point must be resorted to. For ordinary purposes the collimating telescope may be conveniently employed.

(*e*) *Chromatic Aberration.*—The simplest test for this has already been described, *i.e.*, of focusing the distant point of light, and then to observe the appearances produced when the eyepiece is pushed in or drawn out beyond focus. If the object glass is under corrected it will show a red ring round the disc when inside focus, and a blue flare if outside; while if the glass is over corrected the appearances will be reversed. With the most perfect double objective, however, as a rule, absolute colour correction is not obtained owing to the irrationality of the dispersion of the two lenses, and in that case the colours of the rings are respectively greenish yellow and "claret coloured."

Chromatic aberration in the eyepiece on the other hand may be best

*On the Adjustment of Telescope Objectives. T. Cooke and Sons, York. 1896.

detected by turning the telescope so as to view the star almost at the edge of the field. If the objective has been found satisfactory by giving little colour at the centre, and a coloured image is found when viewed through the edge of the eyepiece, the latter is at fault. More refined tests for chromatism are obtained by observing the appearances shown in the diffraction rings with high powers, but this is beyond our present scope.

(*f*) *Spherical Aberration.*—The simplest test for this has also been alluded to under the General Section, but this is insufficient for telescope work. A more sensitive test is to dispense with the diaphragms and to push the eyepiece nearer the object glass, or to pull it out further, in this case, if the spherical aberration is positive, *i.e.*, the edge rays are brought to a focus at a shorter distance, it will be noticed that the disc of light produced on pushing the eyepiece in is not uniform in intensity, but is brightest at the edge, while if the eyepiece is

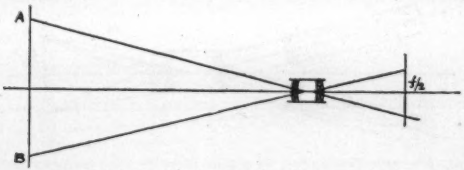

FIG. 12.—DIRECT METHOD OF TESTING EQUIVALENT FOCAL LENGTHS.

pulled out beyond focus the disc will be brighter in the middle than at the edge; the reverse is the case if the spherical aberration is negative by over correction. With high power eyepieces diffraction rings are always visible, but the test is conducted in the same way by noticing whether the inner or outer rings are the brighter.

The Knife Edge Test.—An alternative method of testing a telescope objective is by the knife edge method, which, however, does not seem to offer many advantages, and is more difficult both to undertand and to interpret. Moreover, the knife edge test is only capable of dealing with defects in the object glass, while the former, or direct focusing method, tests the instrument as a whole, which is a great advantage.

Testing Telescope Eyepieces.—With many larger telescopes sets of eyepieces are provided, both astronomical and erecting. The latter especially are liable to many defects owing to the number of lenses of short focus employed in them. It is convenient where different eye-

pieces are used to find the equivalent focal length of each of them, and the magnification of the telescope with each eyepiece may then be simply found by dividing the focal length of the object glass by the equivalent focal length of the eyepiece. The focal length and performance of eyepieces may be obtained exactly as with photographic lenses.

Section III.—Photographic Lenses.

We now pass to the subject of photographic lenses which are distinct from every other class of lens in that very oblique light is to be focused as well as central rays.

Determination of the Equivalent Focal Length.—This has been fully discussed before in the General Section, any of the tests mentioned being suitable. Where, however, any considerable amount of testing is to be done, the writer thinks that the following method will be

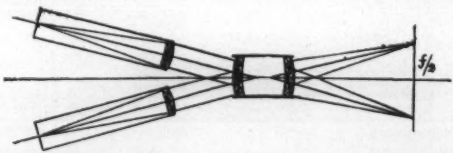

FIG. 13.—DETERMINATION OF FOCAL LENGTH WITH COLLIMATORS.

found most convenient, as it is direct reading and requires no apparatus beyond the ordinary camera. Let two objects A and B, fig. 12, be mounted as far as possible from the camera, which should be fixed in position, and let the distance from A to B be made exactly one half of the distance from the middle point between them to the front of the camera. Under these circumstances if any lens, landscape or combination, is fixed in the camera and the objects are focused, the distance between them when multiplied by two gives the equivalent focal length. For rapid reading a scale reading in half inches or half millimeters may be used. The only drawback to the arrangement is in cases where space is restricted, but this can be easily got over by simply using a pair of collimating telescopes, as in fig. 13, in which case the whole arrangement need not take up a space of more than two or three feet, and the test is perfectly accurate, provided that the lens tested is reasonably free from distortion.

Determination of the Aperture and Values of Stops.—This is of

course a point of very great importance with photographic lenses, and one in which mistakes seem even now to be made. This should not be the case if we had a strict definition of the aperture of a lens, and it may be defined for photographic purposes as the ratio of the diameter of the entrance pupil or largest beam of parallel light which can pass through the lens and stop, to the equivalent focal length of the lens. With single landscape lenses it is of course only necessary to measure the diameter of the stop carefully with a wedge, and to divide it by the distance from the lens to the screen when a distant object is focused. With a combination of lenses, however, two difficulties present themselves, the first being in the determination of the equivalent focal length, which has been already discussed, the second lies in the fact that if a lens is in front of the stop as well as behind it, a larger beam of light can enter the stop than its own diameter, owing to its being converged before reaching it by the front lens. It is of

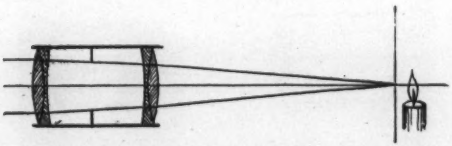

FIG. 14.—DETERMINATION OF APERTURE : KEW TEST.

course possible to calculate the equivalent aperture of the stop by knowing its actual diameter and the focal lengths of the component lenses, but this is troublesome. The most convenient device if the equivalent focal length is known is that employed at Kew, where a distant object is simply focused on the screen, which is then removed, and has then inserted in its place a plain sheet of metal with a pin hole in it, fig. 14. On allowing light to pass through this pin hole back through the lens, a parallel beam emerges from the front of the lens, and the diameter of this parallel beam is clearly the diameter of the largest beam which can pass through the stop. The value of the stop is then given by dividing the diameter of the beam by the equivalent focal length.

The author, however, ventures to suggest an exceedingly simple method of determining the value of the stops of any lens, whether compound or simple. It does not seem to have been pointed out by anyone

that since the aperture of a stop depends upon the ratio of its equivalent diameter to the equivalent focal length, it is really defined by the angle of the cone which the light makes on reaching the screen. In fig. 15 it is easily seen that the aperture or ratio of the diameter of the parallel beam to that of the equivalent focus is equal to the ratio of the diameter of the cone at any point to the distance of this point from the focus. Consequently it is absolutely unnecessary to know either the diameter of the stop or the equivalent focal length, all that need be done is to focus sharply on the screen some distant bright point of light, to then move the screen in a definite known amount, and to divide the diameter of the disc of light then seen on the screen by the distance moved. To make the thing even more simple the screen may be moved until the disc of light assumes a definite diameter, say 1 in. or 1/10th in., and the distance which the screen moves then gives the value of the stop without any calculation whatsoever. For example, if with a

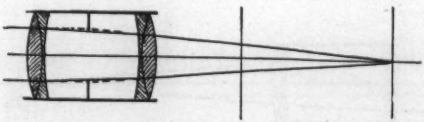

FIG. 15.—DIRECT TEST FOR STOPS.

certain lens a distant lamp is focused, and it is then found that pushing in the screen 4 ins. causes the disc to expand to 1 in. diameter, the aperture is *f*/4. If with a smaller stop pushing in the camera 2 ins. gives a 1/10th in. disc, the stop is *f*/20, etc. This is much more convenient than the Kew method, as it does not require any special screen, the whole operation being performed with an ordinary camera in a few seconds. Of course in many cases the equivalent focal length has to be determined for other reasons, but in any case the determination of the stops can be most quickly made in this way, and frequently a knowledge of the equivalent focal length is unnecessary.

Angle of View.—The extreme angle which the lens can cover irrespective of definition or illumination is immediately found by seeing how far the lens may be turned before the stop is entirely cut off by the lens mount. The covering power, or angle of view, as generally given, however, implies the greatest angle over which the lens may be used at full working aperture, so as to give a certain standard of

definition. The standard usually taken is that the image of a point shall not exceed 1/200th of an inch in dimensions at any part of a flat field.

Only one system of testing photographic lenses, that of M. Moessard, appears to have been devised in which the covering power of a lens is scientifically tested. A special testing camera, known as the Tourniquet, is employed, enabling the lens to be rotated and adjusted to bring either of the nodal points over the axis. The lens is then

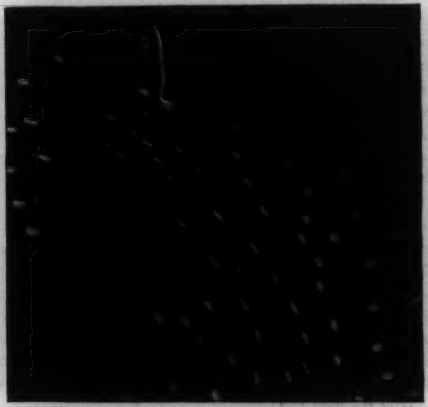

FIG. 16.—SAME AS FIG. 5, LARGER SCALE, SHOWING PRIMARY AND SECONDARY FOCAL LINES, AND CIRCLES OF LEAST CONFUSION.

turned at angles of 10°, 20°, 30°, etc., to the axis, and by means of a micrometer eyepiece, the positions within which the standard definition is maintained, are determined and plotted. A full account of this method is given in Cole's 'Photographic Optics,' to which those interested are referred.

Achromatism.—In testing the achromatism of a photographic lens, it must be remembered that none of the ordinary visual tests are of

any value, as visual and photographic achromatism may be widely different.

The simplest satisfactory test is to photograph an object consisting of a number of cards at different distances from the camera. If the card focused is sharpest in the negative, the achromatism is satisfactory, provided that the screen and plate are in perfect register with one another; but if a nearer one appears sharpest in the photograph the lens is under corrected, or if a further one, over corrected.

Spherical Aberration.—The best test for this is to employ an ordinary eyepiece with the lens, and to test it on a distant point of light exactly as with a telescope.

Coma, Radial Astigmatism, and Curvature of Field.—These three errors concern the falling-off of the definition towards the edge of the plate, and may be considered together. In Moessard's test, before mentioned, these errors are at once apparent, as on observing a distant luminous object with the tilted lens, coma is manifested by the usual one-sided flare, and astigmatism by the light being focused in two focal lines, as with a sphero cylinder, instead of in a point. The curvature of the field is at once apparent on plotting out the focal lengths in various directions.

At Kew the astigmatism is expressed by the diameter of the disc of least confusion with a certain stop, and the curvature by the movement of the screen necessary to bring the edge of the image into focus after the centre has been sharply focused. A better measure of the astigmatism at any angle is the distance between the focal lines compared with the focal length of the lens.

Distortion.—This defect is, of course, easily recognised by observing the image of an object having vertical and horizontal straight lines at the edge of the field. At Kew the amount of distortion of a lens is measured by the sag of the line.

Definition.—A very useful method of estimating the defining power of a lens is used at Kew, in which a thin strip of steel is set up at a distance from the camera in front of a well-illuminated screen. When this strip is edge on it is invisible, but as it is turned round a point is found at which it is just visible on the screen, and the better the lens the less will the strip require to be turned.

Simple Methods.—The tests above described enable one to test a lens with a fair degree of accuracy, but for ordinary purposes they are somewhat troublesome. It will, therefore, be well to describe one or two methods by which the Optician can get a general idea of the per-

formance of a lens. A common one, as used by English Opticians, is to simply photograph a strip of some printed matter, some simple geometrical designs being included with the lettering; the centre of this strip is carefully focused with the lens, and a photograph is afterwards taken on a large plate. The extreme angle of view of the lens is then indicated by the distance of either side of the centre to which the image extends; the uniformity in illumination by the density of the negative, spherical aberration, distortion, astigmatism, etc., by the quality of the definition, the straightness of the lines, and the ability to see both horizontal and vertical lines equally distinctly. A com-

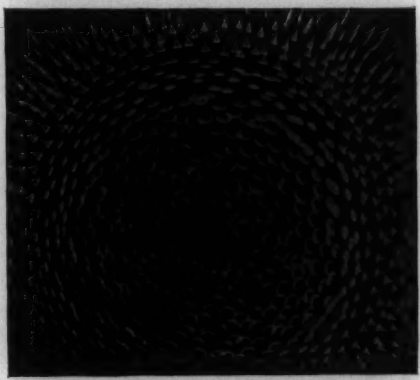

FIG. 17—COMA.

parison of photographs so obtained with various lenses gives a fair idea of their performance.

The method of testing devised by Dr. Paul Rudolph, of Jena,* is one that seems to be little known over here, and which is unquestionably both simple and capable of considerable accuracy. In it the old device of fixing a number of cards on a spiral round a rod has been

*Uber eine Neue Methode zur bildlichen Darstellung der leistungsfahigkeit Photographischer Objective. von Dr. Paul Rudolph. Ateuer des Photographen 1894 Heft 8.

employed, as in the above described test for chromatic aberration, the centre card being focused, and observation being made of the most sharply defined card in the negative. Dr. Rudolph makes use of a number of such sets of cards spaced out at equal distances horizontally, and having beneath them an evenly divided scale with geometrical designs on it. When a photograph is taken of this arrangement the scale indicates the covering power, etc., as with the test above mentioned, while any curvature of the field is indicated by the fact that different cards of the various sets are focused. By knowing the dis-

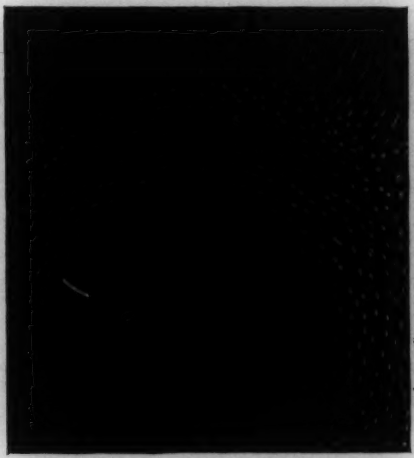

FIG. 18.—RADIAL ASTIGMATISM.

tances of the cards apart, etc., it is possible to map out the form of the field with a fair degree of accuracy. This device is one which anyone can easily make up, and the information given by it is exceedingly valuable. Moreover, if two of the marks on the scale are placed at a suitable distance apart with respect to the camera, it will be possible to include in the arrangement the simple test for the equivalent focal length proposed by the author.

Section IV.—Projection Apparatus.

Little need be said about the testing of Projection Apparatus by itself, as practically all that can be said about it has been covered in the section relating to photographic apparatus. The projection lantern consists essentially of two parts, the condenser and the projection lens. The function of the latter is exactly the converse of that of the photographic lens, and consequently it should be tested in the same manner, the only difference being that as a rule the angle of view required with the projection lens is rather less than with the camera. Concerning the condenser, its only function in a good projection lantern is to focus the light proceeding from the radient as nearly as possible to a point at the projection lens, and it is, therefore, only advisable that it should be as free as possible from chromatic and spherical aberration on the axis. Unfortunately, however, there is hardly any piece of optical apparatus which sins to such a grievous extent as the condenser of a projection lantern. For ordinary slide projection the quality of the condenser is of secondary importance, but if the lantern is to be used for scientific projection work the condenser cannot be too carefully constructed, and the writer has never met with a condenser which even approximately fulfilled the conditions required. The defects of this part of the apparatus are usually so serious as to cause a loss of at least three-fourths of the light in any experiment involving the projection of a small aperture or a slit. It is greatly to be hoped that manufacturers will do something to remedy this state of things.

Section V.—Microscopes.

In entering on the subject of the testing of microscope lenses we are brought face to face with the difficulty at the outset that no definite recognised system of testing seems ever to have been adopted, with the sole exception of the determination of the aperture. It seems curious that in the case of so accurate and costly an instrument as a good microscope objective there should be absolutely no standard by which the attainments of it can be judged. Several methods of testing, notably the mercury test, the grating test, and the Abbe test plates have been in vogue, but all these tests only serve to discriminate between the comparative merits of different objectives under test at the same time, and give no absolute indication of the extent of any errors present. No doubt the extremely short focal length and wide aperture of many objectives make the task of testing appear formidable, but the

writer ventures to think that the problem is not incapable of solution, and to suggest a method by which such results can be obtained.

Before, however, saying anything about a new scheme of testing it may be well to briefly refer to the ordinary methods.

Determination of the Equivalent Focal Length.—No very satisfactory method of obtaining this seems to be given in the books devoted to microscopical subjects. The most common method appears to be by finding the magnification of the image of an object micrometer produced with a known tube length, the difficulty with this in practice is that the tube length ought really to be measured from the nodal point of emergence of the objective, the position of which is not ascertained, and a considerable error may, therefore, result. A modification of the test, devised by the writer, is to insert in the microscope tube, in place of the microscope objective, a telescope of known magnification, and to ascertain the size of the image in the focal plane of the telescope produced by the stage micrometer. In this case, since the rays emerge parallel from the back of the objective, the position of the nodal point is immaterial.

Determination of the Aperture.—This is always effected by the exceedingly beautiful apertometer devised by Professor Abbe, and which is well known. It consists of a semi-circular block of dense glass, on the circumference of which two pointers slide. The straight edge of the block is bevelled at an angle of 45° so as to throw light proceeding from the edge upwards as if it had come from a semi-circular scale lying vertically below the glass instead of to the side of it. The objective to be tested is focused on a small aperture in the centre of the straight edge, either dry or with its appropriate immersion, so that light reaches it from all parts of the semi-circular edge. On withdrawing the eyepiece or inserting a lens into the tube so as to bring the edge of the block into focus, the pointers can be seen, and by sliding them round the block a position is reached where they just appear to be at the edge of a circular field. A scale is provided on the block, the readings of which represent the sines of the angles which these divisions make with the axis multiplied by the refractive index of the block, and which, therefore, gives the values directly of the numerical aperture of the objective. The test is expeditious and very accurate, the only condition required to read conveniently being that the glass block should receive diffused light from all directions, and not from a single source, otherwise the pointers will not be visible.

Determination of Errors.—The most common method of testing a

microscope for defects in this country has been by the mercury test, in which a small globule of mercury is placed on the stage and is crushed with a flat surface so as to split it up into exceedingly small globules, each of which when viewed by reflecting light has a small point of light on its surface, and, therefore, acts as an artificial star.

The tests for achromatism, spherical aberration, etc., can be performed on this precisely as with telescopes. A globule is first taken in the centre of the field and the appearances are viewed when the microscope is either focused downwards or upwards; if in the former case the disc appears with a red fringe, and when outside the focus with a blue flare, the objective is under-corrected. If the opposite appearance is viewed, it is over-corrected, while the appearance when the colour is properly corrected for is the same as with a properly corrected telescope object glass. It must be remembered, if an apochromatic lens is being tested, the test must be performed with its own compensating ocular, and in that case an almost perfectly colourless disc should be viewed either inside or outside the focus. Spherical aberration can, of course, be seen by noticing whether the disc is uniform in brightness or not, as with telescopes.

The same tests may be performed with a globule at the edge of the field, in which case coma or astigmatism may be noticed.

The mercury test is unsuitable, however, for objectives of very small focal length and large aperture, partly from difficulties of illumination and mounting, but chiefly because there is no means of varying the aperture with which the objective is employed. With the exceedingly large aperture of 1·4 which is now reached, even the best objectives fall off markedly in definition with the extreme rays, and it is consequently not always advisable to work with the full aperture.

Professor Abbe has introduced "test plates" in which lines are ruled on a silvered surface, and covered with cover glasses of various thicknesses. These objects are viewed by transmitted light in the ordinary way with a carefully centered condenser, the aperture of which can be varied at will by a diaphragm, and the appearance of the bands noticed on focusing up and down. These plates are particularly suitable for adjustable objectives, enabling one to find the best correction with considerable accuracy. By using also various stops in the condenser so as to give axial or oblique light, spherical or chromatic aberration can be detected.

Suggested New Method of Testing.—This paper is already so long that a few words only will be devoted to a new method of testing

microscope lenses which the author ventures to suggest. It is founded on the principle of auto-collimation, first used by Professor Abbe in his spectrometer, and independently devised by the writer for curvature measurement, and consists in making the light traverse the whole system or any part of it twice. This obviates all question as to centering and makes one independent of the quality of a condenser.

If we use an ordinary microscope, provided with the objective and eyepiece to be tested, and simply fix a small piece of plane, unsilvered glass above the eyepiece, the light from any distant small source of light can be reflected down the system, and will be converged by the objective. If we now use the ordinary Abbe test plates simply as reflectors, the light will be returned by them up the tube, and if the microscope is focused in the ordinary way, an image of the distant light will be seen. The appearances on focusing up and down can now be studied exactly as with an ordinary telescope, with the advantage that every defect is, of course, doubled in its effect. Not only so, but it makes it easy to have an absolute standard of defining power. With the ordinary methods, exceedingly small objects have to be employed on the stage, but this is unnecessary with the method proposed, as the visual angle between the different parts of the object is not altered by its having passed twice through the system. Consequently, ordinary test types may be employed, and with a perfect microscope the visual acuity should be the same as if directly viewed from the same standpoint, provided that the type is sufficiently illuminated to make up for the loss of light.

In order to make this test with the greatest convenience, a special microscope has just been made at the Northampton Institute, which, besides having the reflecting device, is provided with a horizontal axis carrying the objective, with an adjustment for bringing the nodal point on the axis. The objective can then be tilted at any angle, allowing axial or oblique light to be dealt with easily, and, further, allowing the focal length to be conveniently determined. Time has not permitted us to make much use of this instrument as yet, but I am inclined to think that we shall be able by its means to investigate microscope lenses much more thoroughly than by other methods. Further, as the necessity for small objects is done away with, I have some hope that it will enable us to investigate the much vexed diffraction theory more easily and completely.

The auto-collimation principle appears to me to be most valuable in all testing work. It has already been adopted by Dr. Clay for

photographic lens testing, and it might possibly be employed with advantage in telescope work, if a perfectly plane mirror were fixed in front of the object glass.

In bringing this somewhat lengthy paper to a close, I can only hope that in spite of its acknowledged many imperfections, it may be of use to many Opticians and serve as a stop gap until something more complete is available. I also venture to hope that some may be led to study the subject more closely, and to devise new methods or improvements on existing ones, which there is unquestionably good opportunity and necessity for. We shall never be able to ensure the best work in optical instruments until methods of testing are available by which the important quantities and errors of every instrument can be *numerically* stated. Up to the present, hardly anything has been done in this direction, except in the case of photographic lenses, and that only partially. It is much to be hoped that our National Physical Laboratory, or some other recognized institution, will be able to take up this work in the near future.

Finally, I must express my best thanks to Mr. Jolley and Mr. Greenfield, for designing the details of and constructing the testing microscope referred to, and to Mr. Mariner for the great assistance given in the experiments you have seen.

PRISMS AND THEIR NOTATION.

Paper by Mr. Lionel Laurance, *Member.*

Read before the Optical Society, July 24th, 1902.

THE prism used in visual optics is generally made of glass, and is round, oval or square as to its perimeter, and has two refracting surfaces inclined towards each other. The thin part of the prism is called the apex, or, more conveniently, the edge, and the thick part is the base.

As we all know so well, light, when passing obliquely from a rare into a dense medium, is bent towards the perpendicular and, when passing from a dense into a rare medium, is bent away from the perpendicular. And since no incident light can be perpendicular to two surfaces which are inclined towards each other, it follows that any ray of light, no matter what may be its direction of incidence (provided that the angle of incidence at the second surface be not greater than the critical angle), must be refracted by a prism, so that its ultimate direction on emergence will be altered towards the base. As the emergent light is deviated towards the base, so the apparent place of an object viewed through a prism is deflected towards the edge.

If the angle of incidence is the same for the various rays contained in a beam of light, they are all—for practical purposes—equally refracted by a prism and are, therefore, deviated to the same extent, no matter on what part of the first surface they may fall; consequently, rays which are parallel before refraction are similarly parallel after refraction.

When a pencil of light is divergent from a point, or convergent to a point, all the rays comprised therein cannot be incident on the first surface of the prism at the same angle, so that some suffer more deviation than others, and on emergence the pencil of light is more or less convergent or divergent (although unequally so in the different meridians) than it was before refraction; yet as the pencils are small and the prisms we use in spectacle work are of low power, divergent

and convergent pencils of light may be regarded as being equally divergent or convergent after refraction as they were before. I mention this because in practice we may—otherwise than as with lenses—use divergent instead of parallel light for the measuring of prisms.

The plane which bisects the angle at the edge of the prism is termed the base-apex plane. Any line lying in this plane, and which is at right angles to the edge, is termed a base-apex line, but usually we take the central line of this plane to be *the* base-apex line.

A line which lies in the base-apex plane midway between the edge and the base, and parallel to them, may be regarded as the axis of the prism.

On looking at a cross-bar through a prism and rotating the latter in

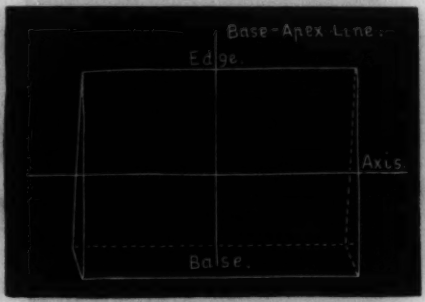

FIG. 1.

the base-apex plane around a line at right angles to and bisecting its axis—that is, around the geometrical centre of the prism—it is found that the two lines of the cross-bar apparently move; the vertical line moves in the horizontal plane, and the horizontal line moves in the vertical plane, but the two lines remain always at right angles to each other. In a certain position of the prism, one of the lines is seen continuous within and beyond the extremities of the glass; the continuous line is then parallel to the base-apex lines—the position of which is thus located.

The two refracting surfaces of a prism, being inclined towards each other, form an angle at the edge, which is called the angle of inclina-

tion or the refracting or principal angle; it is commonly expressed as of so many degrees or (and) fractions of a degree.

The ordinary notation of prisms is based on the value of the refracting angle, but this method of numeration simply gives the form of the prism and does not express its optical effect, for, of course, the extent of bending, which light undergoes when passing from one medium into another, depends not only on the angle of incidence, but also on the difference between the optical densities of the two media, that is to say, if light passes at a certain obliquity, from a rare into a dense medium or *vice versâ*, it is bent respectively towards or

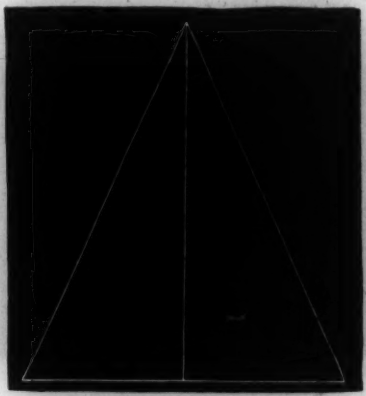

FIG. 2.—THE BASE-APEX PLANE.

away from the perpendicular to an extent governed by the difference in the indices of refraction of the first and second media.

Then the amount of deviation which a ray suffers, when passing through a prism, depends partly on the refracting angle or angle of inclination of the two surfaces, and partly on the index of refraction of the glass of which it is made. Therefore if the notation of the prism is indicative of but one of the true properties which combine to give it an optical value, such notation, obviously, must be inadequate and misleading. The expression—by means of the refracting angle—of

the physical form of the prism, would, it is true, suffice, if optical glass were always of the same index of refraction; but this is not the case. The numeration of prisms according to the refracting angle may be compared to the numeration of lenses according to the radius of curvature; the focal lengths of two lenses similar as to thickness and curvature, the one being made of glass $\mu = 1{\cdot}6$ and the other $\mu = 1{\cdot}5$, are very different, and a like difference exists in the optical effects of two similarly angled prisms whose μ's are respectively 1·6 and 1·5, the former is about 1/5th stronger than the latter.

In order to learn the refracting angle of a prism its sides may be enclosed between the legs of a pair of compasses and the angle, thus obtained, measured on a protractor. Or the angle can be read off from a specially constructed apparatus. The prism can also be neutralised by a prism of known strength from the trial case, but this, of course, would only be approximate, so far as the refracting angle is concerned, for what is then neutralised is the angle of deviation. Or the deviating angle can be measured and the refracting angle calculated by a formula, which I shall give later, if the index of refraction of the glass be known.

If a distant object be looked at, through a prism, it is apparently displaced towards the edge, for the light from the object, being refracted by the prism, is bent towards the base, and entering the eye in this direction is referred back according to the well-known law of projection. The mental image is referred to the object side of the prism to a point nearer to the edge; and if the course of a certain incident ray be continued until it cuts the corresponding emergent ray, which has been referred back, we shall find that the point of intersection of the two rays is in the centre of the prism. The angle formed by the incident and emergent rays is termed the angle of deviation, and prisms are numbered also according to the size of this angle.

The extent of the deviation of the incident ray towards the base depends, independently of the prism angle and index of refraction, on the angle of incidence of the ray. If, after refraction at the first surface, the ray takes a direction parallel to the base of a symmetrical prism, or when the angles of incidence and emergence are equal so that the point of incidence and the point of emergence are equi-distant from the edge, then the ray suffers less actual deviation than if it were incident at any other angle whatever—whether greater or smaller. The position of a prism when parallel rays of light suffer the least possible deviation is termed the position of minimum deviation.

When the deviating angle of a prism is indicated it necessarily has reference to that of minimum deviation. A prism numbered according to its deviating angle is said to be of so many degrees d in order to distinguish it from one numbered according to the value of the refracting angle.

The deviating angle of a prism is equal to the sum of the deviations at the two surfaces, and the calculation involved for finding it, when the refracting angle and the index of refraction are known, is sufficiently complex. It is,

$$2 \sin^{-1} (\mu \sin P/2) - P = d$$

the deviating angle is equal to twice that angle whose sine is ($\mu \sin P/2$) less the principal angle of the prism.

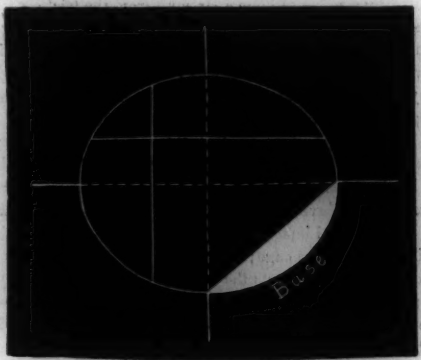

FIG. 3.—OBLIQUE DISPLACEMENT BY PRISM.

This formula is taken from Dr. Maddox's "Ophthalmological Prisms," a work which I can earnestly recommend to anyone interested in the subject here treated.

If, however, angles are taken in place of their sines, as may be done when prisms are of small angle, the formula becomes very simple. It is:

$$P (\mu - 1) = d$$

where P is the principal, and d the deviating, angle of the prism. If the index of refraction of the glass is 1·5 the deviating angle—when a

minimum—is half the principal angle, and this is usually the accepted equivalent; but if the index of refraction is higher, the deviating angle bears a greater ratio to the principal angle; thus, with a 10° prism if $\mu = 1{\cdot}5$ the deviating angle is 5°, while if $\mu = 1{\cdot}6$ the deviating angle is 6°.

And since the deviating angle of a prism varies with the refracting angle and the index of refraction, it follows that a prism of smaller refracting angle, and higher index of refraction, may have the same deviating angle as one of greater refracting angle but lower index of refraction.

The angle of deviation itself cannot be directly measured, but the angular displacement caused by it can be quite easily by means of some special apparatus, such as the spectrometer described in Dr. Percival's work, or in "Ganot's Physics," or such displacement can be measured by the tangent of the angle, the distance between the prism and the object being taken as the radius.

Thus in order to measure the deviating angle of a prism a tangent scale, as designed by Dr. Maddox, is used, the scale being marked in tangents of degrees for the given distance at which the prism is held, say three meters.

It is easy to see that the space measured by the tangent of an angle is directly proportionate to the distance between the tangent and the vertex, so that if a scale is made for 3 m., the apparent displacement shown on it would be double if that scale were used at 6 m., and would be only half the amount if used at 1·5 m.

Or a centimetric scale can be used, and if the zero line is apparently displaced to No. 6 (cm.) when the prism is 3 m. distance from, and directly opposite to, the datum line the displacement is 6 cm. at 3 m. and $6/300 = \tan d$; in this case ·02 — the angle of deviation is approximately 1° 9′.

There are two points to be carefully observed when using a tangent scale, they are:—

1st. That the base-apex line shall be exactly horizontal; this is known to be the case when the black horizontal line of the scale is continuous within and beyond the prism. If the base-apex plane is inclined the actual deviation, indicated in the horizontal plane, is less than the deviating power of the prism, for part of the prismatic effect is then expended vertically, or more exactly, the maximum deviation being oblique, the horizontal deviation is not so great as it would be if the prism were exactly horizontal.

2nd. That the deviation shall be minimum; this is the case when by slightly rotating the prism on its axis, both ways—while looking through it—it is found that the actual deviation is in each case increased. The position of least deviation is what is needed; any departure from it, so that the base or the edge of the prism is more advanced towards the scale, causes the vertical pointer or zero line of the scale to indicate a higher number than the true deviating angle of the prism.

Notation of prisms, according to the deviating angle, is infinitely preferable to that according to the principal angle. At least the true optical effect is indicated. Moreover, deviations of the visual axes

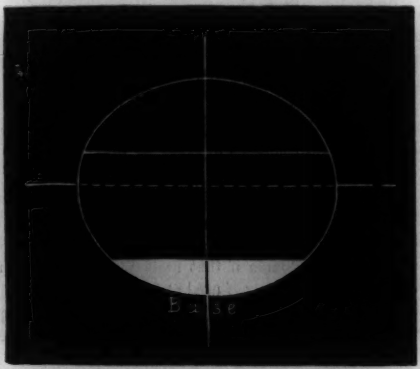

FIG. 4.—BASE-APEX LINE VERTICAL.

which prisms are intended to produce or maintain are also expressed in degrees. Also degrees of deviation can be added or subtracted without error—a 3° *d* and a 2° *d* equal a 5° *d*. One disadvantage is that unless a tangent scale is available considerable calculation and recourse to a table of tangents becomes necessary in order to learn the value of a prism in degrees of deviation; also, tangents cannot be added together—the tangent of 2° is more than double that of 1°—although the error here involved is hardly appreciable with the weak prisms such as Refractionists have to deal with. The chief disadvantage is that the relationship between the degree of deviation and

the meter-angle is confusing. For while deviation of the visual axes is expressed in degrees, convergence is expressed in meter-angles; the deviation, being a variable quantity dependent on interpupillary distance, is disregarded when convergence is discussed in practice.

The third method of numeration of a prism is based on the actual deviation effected by it. This presents many advantages, especially as regards simplicity of calculation; it is the invention of Mr. Charles F. Prentice, of New York. The unit is called the prism diopter, and it has that prismatic power which causes, at a distance of one meter, a deviation of 1 cm. measured on a flat surface; it is equal to about ·57° or 34 1/2′. The deviation is one in 100, and prism diopters

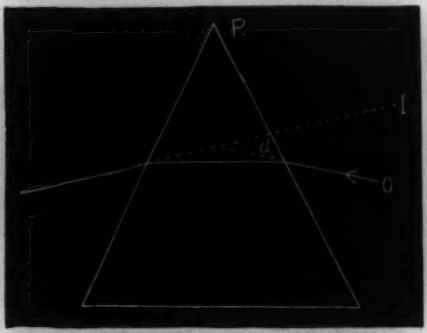

FIG. 5—PRINCIPAL ANGLE AND ANGLE OF DEVIATION.

divided by 100 give the tangent of the angle of deviation. The prism diopter is, therefore, that angle whose tangent is one hundredth part of the radius. But since tangents increase more rapidly than the angles themselves, separate prisms numbered in the prism diopter scale, when placed together, are not equal to the sum of their individual numbers. A 3 △ and a 2 △ are not exactly equal to a 5 △. The difference here, as I before stated, is, however, so small in the weak prisms used in visual optics that it can be disregarded.

The prism diopter is measured on a tangent scale in the same way as the degree of deviation, only instead of the divisions being tangents of integral numbers of degrees they are each of as many centimeters

as is the distance in meters at which the scale is to be used. Thus each division would be of 3 cm. or of 4 cm. if the scale were made for use at, respectively, 3 m. or 4 m.

The connection between the prism diopter and the M A is simple enough if we take sines and tangents as being equivalent to each other. A prism of as many diopters as half the interpupillary distance has centimeters, causes 1 M A convergence of one eye. Thus if the interpupillary distance is 6·4 cm., half that distance is 3·2 cm., and the prism which causes such a deviation at 1 m. is one of 3·2 △. A pair of such prisms would cause or prevent 1 M A convergence of the two eyes.

Above all things the prism diopter lends itself to easy calculations in connection with the decentering of lenses and than which nothing can be more simple. In this respect this system is greatly superior to the others mentioned.

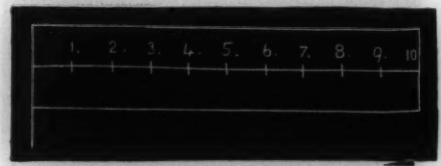

FIG. 6—TANGENT SCALE.

The centrad is yet another unit of measurement of prisms, it is similar to the prism diopter in that it corresponds to a deviation of one in 100; only such deviation is measured on the arc of a circle instead of on a flat surface, it is equal to about the same proportion of a degree as the prism diopter. But arcs and tangents of small angles are so nearly equal in value that any difference between these two units may be disregarded. The value of the M A in centrads is practically the same as in prism diopters, but as the meter angle is a sine measurement there is less difference between it and the arc than between it and the tangent. Centrads can be added together without error; for arcs are directly proportionate to the angles they subtend. Centrads divided by 100 gives the arc of the angle of deviation. One inconvenience is the difficulty of arranging curved scales for the measurement of the centrad.

The degree of deviation, the prism diopter and the centrad, being

different measurements of the optical effects of a prism, the angle of inclination or principal angle varies inversely with μ for given powers expressed in any of these systems of notation. It is, therefore, impossible to express a simple relationship between them and the principal angle system of notation.

The prism diopter has, however, about the same value as the ordinary prism degree when the index of refraction is 1·54, the degree is then ·54° of deviation or about 32′ 24″ and the prism diopter is ·575° or 34′ 30″. There is a difference of about six per cent., the latter being so much stronger than the former. The prism diopter causes a deviation of 1 cm. at 1 meter and the degree when $\mu = 1{\cdot}54$ causes ·94 cm. — for tan. 32′ 24″ is ·0094.

The principal angle of a prism required to produce a 1 $\triangle$ when μ is 1·54 is about 1° 4′. If μ is 1·575 the prism degree equals the prism diopter, since ·575° d = 34′ 30″ the tangent of which is ·01.

The respective values of the various unit prisms can be expressed in actual deviation at 1 m.

1° d	=	1·745 cm.
1°	=	·94 „
1 $\triangle$	=	1· „

For a constant for degrees I take the index of the glass as 1·54, for if not exactly that which is most generally used for lenses it is at least that which is most generally used for calculations.

Therefore:—

1° d	=	1·85°	=	1·745 $\triangle$
1°	=	·54° d	=	·94 $\triangle$
1 $\triangle$	=	·575° d	=	1·06°

Conversion from one system into another can be made by employing these constants.

The last unit of prism notation that I shall mention—and I propose to ask your attention to this in a little more detail presently—is the meter angle. The meter angle is the angle of convergence of each eye at 1 m., and is, therefore, the angle subtended at the centre of rotation of the eye by the space over which the visual axis passes when moved from the primary position of fixation for ∞ to that of fixation at 1 m.

Since the interpupillary distance varies, it follows that the actual angle of convergence and, therefore, the actual value of the meter angle differs for different people, and in order to cover all cases the meter angle is defined by Dr. Maddox as that angle whose sine is half the interpupillary distance in terms of meter. The nearest approxi-

mate to a fixed value which we can get is if we take it that the interpupillary distance is 64 mm.; then the meter angle equals 1° 50′—for half 64 is 32 and 32/1,000 equals ·032 which is the sine of 1° 50′. This is the commonly accepted value of the M A.

When the interpupillary distance is 58 mm., half this is 29 and 29/1,000 = ·029 which is sin 1° 40′ approximately; so that where the interpupillary distance is 58 mm. the value of the meter angle is 1° 40′. When the distance between the centres of the eyes is 60 mm. the M A = 1° 45′ very nearly. Meter angles, being measured by their sines, cannot be added to one another.

Conversion of meter angles into degrees of deviation, degrees of the refracting angle and prism diopters can be effected by multiplying the meter angles respectively by 1° 50′, by 3 1/2 and 3·2, but all such

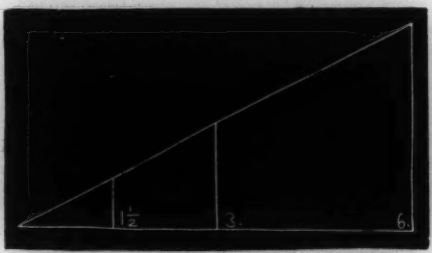

FIG. 7.—DISPLACEMENT PROPORTIONAL TO DISTANCE.

calculations are, of course, approximate only, for the reason mentioned that the meter angle is not a definite quantity.

Moreover, as regards the degrees of the refracting angle, conversion is still more approximate only, on account of variations in the index of refraction of the glass.

With regard to prism diopters we have to take sines and tangents as equivalent when converting the meter angles. If the object be exactly 1 m. from the centre of the prism and appears deviated on a flat surface, its angular displacement is measured by the tangent, and its apparent distance is greater than 1 m. from the prism. If the object, when apparently displaced, is exactly 1 m. from the prism then its original position was less than 1 m. distant. When convergence for any distance is referred to, it is the distance measured obliquely

from the eye to the median line and not measured directly in front. Therefore the distance is the hypothenuse side of the triangle, and the distance between the centre of the eye and the centre of the base line measures the sine of the angle of convergence. To put it plainly, if we measure angular displacement by the tangent, the real distance of the object is the radius, if we measure by the sine the apparent place is the radius. Thus when convergence for 20 cm. takes place con. = 5 M A and we should say that it is equal to $5 \times 3{\cdot}2 = 16$ △. The angular value of 16 △ is 9° 6′, whereas the actual convergence is 9° 13′ for an interpupillary distance of 64 mm.

And even with degrees of deviation errors arise when converting from one to the other. Thus if 1 M A = 1° 50′ we take it that the 5 M A convergence, exerted for fixation at 20 cm., is equal to 1° 50′

FIG. 8—RELATIVE DISPLACEMENT OF VARIOUS UNIT PRISMS.

$\times 5 = 9° 10'$ whereas, as just mentioned, the value of 5 M A is 9° 13′ for $3{\cdot}2/20 = {\cdot}16 = \sin 9° 13'$. The angle of convergence is more than proportionately greater for shorter distances.

These differences are, however, in the prisms we use, not of appreciable importance.

While the arc varies directly with the angle itself, the sine increases less rapidly and the tangent increases more rapidly than the angle. So if a prism is numbered by a unit based on an arc measurement addition of such units gives the true value of the sum of their powers; this is the case with the centrad.

If numbered according to a tangent measurement, the unit being a prism the tangent of whose angle of deviation is of a certain value, as is the case with the prism diopter, then say two of these unit prisms

placed together have a greater angular effect than a 2△, or a 2△ is less than double the value of the 1△.

If numbered according to a sine measurement, the unit being a prism the sine of whose angle of deviation is of a certain value, as is the case with the meter angle, then say two of these units placed together have a smaller effect than a 2 M A, or a 2 M A is more than double the value of the 1 M A.

Of course the inability to add (*ad-libitum*) units together and obtain a corresponding value is limited not only in prisms but in lenses.

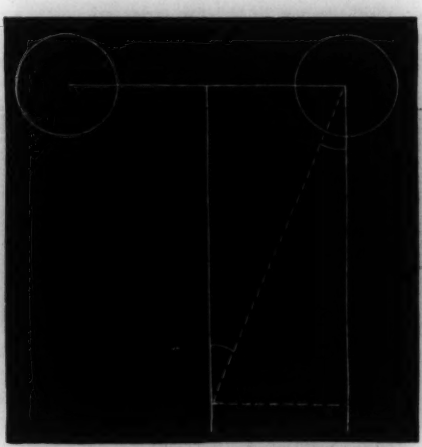

FIG. 9.—THE METER ANGLE.

We commonly say that two ordinary 1 D spectacle lenses are equal to a 2 D or that a + 1 and a — 1 neutralise each other, neither fact is strictly true unless the latter be planos with their curved surfaces in contact.

An ideal notation for prism values would be

(*a*) One which possesses the qualification of permitting multiplication and addition without error.

(*b*) One with which decentration calculations are simple.

(*c*) One which bears a true relationship to the meter angle of convergence.

(*d*) One which is easily convertible into degrees, and

(*e*) One which admits of easy practical measurement.

I think that an attempt to find such a notation would be futile, but we might certainly employ that method which, although not ideal, is at least, in some respects, rather better than others in use.

If the distance between the centres of the eyes be taken as a fixed quantity of 60 mm., which is an extremely fair average, it being equal to about 2 3/8 ins., half of it is 30 mm., and this is the space over

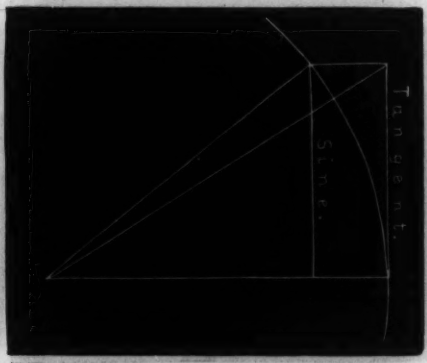

FIG. 10.

which the visual axis passes from the primary position to convergence at 1 m.

Now a prism which causes such a deviation might evidently be taken as the unit measure, and if we adopt this unit we might call it the metran. It is as nearly as we want it for practical purposes a prism whose angle of deviation is 1° 45′, and we might express the metran, like the meter angle, as that angle whose sine is measured by half the interpupillary distance, or it is that prism which causes at a distance of 1 m. a deviation of 3 cm., on a flat surface.

A pair of metran prisms placed base-in before the eyes would relieve convergence entirely at 1 m. If placed base-out when the eyes are

fixed for ∞ they would cause one meter angle of convergence. The metran is equal to the meter angle for the average interpupillary distance of, say, 60 mm., or more justly for about 61 mm., since if the distance between the centres of the eyes be 60 mm., the M A is more nearly 1° 43′ 45″.

And since the metran causes a deviation of 3 cm. at 1 meter distance, it is of three times the strength of the prism diopter, or rather it is equal to 3 △.

I think that the metran presents some advantage over the prism diopter, in that the notation closely corresponds to the meter angle, indeed it is but an adaptation of that measurement and so calculations are facilitated, and we preserve a more nearly integral angular value for the unit.

The angular value of the metran being 1° 45′ or 1 3/4°, its value

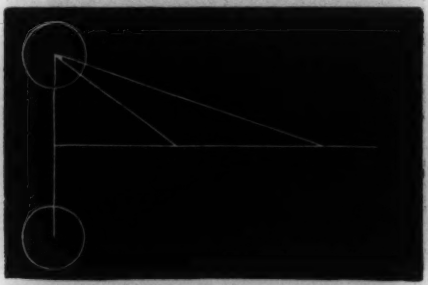

FIG. 11.—CONVERGENCE FOR 50 CM., AND 25 CM.

expressed in angle degrees is about 3 1/2 for $\mu = 1{\cdot}50$. The metran can be subdivided decimally or into three parts which might be called dioptrans.

The dioptran being ·33 metran is practically the same as the prism diopter, and the name seems to me to be a little more terse. The prisms at present in use in trial cases would serve for this notation, the 1 △ being then a dioptran and the 3 △ being the metran. If prisms are numbered in degrees a 3° may without serious error be taken as equivalent to the metran.

The symbolic expression of the metran might be a small angle thus < or ∧ while the symbol △ might be retained for the dioptran.

The metran would be measured on a tangent scale, each division being 3 cm., for a scale to be used at 1 m. distance, and for other distances in proportion. But as the minimum tangential deviation of 1° 45′ is 3·05 cm., at 1 m., I would suggest that the distances should be taken as between the eye and the tangent scale, the prism being held close to the eye at the usual place where a spectacle lens is generally put, that is, say, 18 mm.; then the distance between the

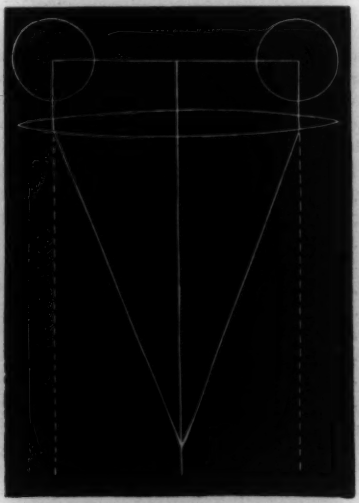

FIG. 12.

prism itself and the scale is a trifle less than 1 m., say 98·2 cm., at which distance the tangent value of 1° 45′ may be said to be 3 cm., exactly.

A 1 D lens decentered 3 cm. would have the same prismatic effect as if a 1 < had been added to the lens. A sufficiently large + 1 D lens held with the optical centre in the middle line of the face and looked through would cause neither accommodation nor convergence to be exerted at 1 m. The accommodation is relieved by the dioptric power

of the lens and the convergence by the prismatic effect of 1 < base-in, which is obtained from the optical centre of the lens being 3 cm. distant from the centre of each eye. A large + 3 D lens would have a similar effect for 33 cm., and so on.

Calculations for decentering would then be on the basis of 3 cm. for each metran for each diopter of refractive power. Expressed symbolically this is :—

$$\frac{D \times C}{3} = < \quad \text{or} \quad \frac{3 <}{D} = C$$

where D is the power of the lens in diopters and C represents centimeters or parts thereof. For the dioptran the same method would be retained as is now in vogue.

As the angular deviation of the metran is 1° 45′ the metran prism would need be made to an angle varying with μ as follows :—

μ =	1·50	angle of prism	3° 30′
„	1·51	„	3° 26′
„	1·52	„	3° 22′
„	1·53	„	3° 18′
„	1·54	„	3° 14′
„	1·55	„	3° 10′
„	1·56	„	3° 7′
„	1·57	„	3° 4′
„	1·58	„	3° 1′
„	1·59	„	2° 58′
„	1·60	„	2° 55′

The calculations are made from the formula

$$\frac{d}{\mu - 1} = P$$

which serves perfectly well for calculating the angle at which a prism should be ground in order to obtain a certain deviating power. In it angles are taken instead of their functions.

In the same way as a pair of + 1 or + 2 D lenses relieves accommodation each to that extent, so we should be able to say that a pair of 1 or 2 metran prisms base-in relieves convergence to the extent of 1 or 2 M A respectfully. As, say, a pair of — 3 D lenses brings accommodation of 3 D into action, so a pair of 3 metrans base-out causes 3 M A convergence.

Since we use the term plus for positive lenses which supply a de-

ficiency of refraction or of accommodation in the eyes so we might call prisms base-in positive or + for they relieve convergence or supply a deficiency of convergence; prisms base-out we might designate as negative or —, they increase convergence or neutralise an excess of that function. At 33 cm. distance muscular efforts of emmetropic orthophoric eyes would be rendered unnecessary by putting in front of each a + 3 D sph. ⌒ + 3 < while — 3 D sph. ⌒ — 3 < would cause similar eyes when fixed for ∞ to exert their functions as if viewing an object at 33 cm.

Then my definite propositions are:—

(*a*) That our standard unit prism be one whose angular value is 1° 45′ and whose deviating power is such as to cause 3 cm. deviation when held close to an eye which is distant 1 m. from the object viewed.

(*b*) That prisms be measured on a tangent scale whose divisions are 3 cm. for each meter at which the scale is used.

(*c*) That multiples and sub-multiples of the deviation caused by the unit prism be considered of angular values directly proportional to such multiples and sub-multiples.

(*d*) That the unit prism be called the metran.

(*e*) That the symbol of the metran be a small angle placed in the usual position for such symbols.

(*f*) That the metran be regarded as equal to the meter angle for all ordinary interpupillary distances.

(*g*) That a 1 D lens decentered 3 cm. be considered to have the prismatic effect of 1 < and other lenses and other distances be considered proportional.

(*h*) That a prism base-in be called a plus prism, and its position indicated by a + sign; that a prism base-out be called a minus prism, and its position indicated by a — sign.

(*i*) That all deviations of the visual lines whether vertical or horizontal be expressed in terms of the metran.

In conclusion I trust that in attempting to deal simply with what is to me really a difficult subject any shortcomings of mine or any errors I may have made will meet with that kindly consideration which the members of our Society are in the habit of showing.